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Psychological Bulletin

IMPROVEMENT IN PERCEPTUAL JUDGMENTS AS A FUNCTION OF CONTROLLED PRACTICE OR TRAINING[1]

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In spite of a long history of research on problems of learning, current textbooks contain little information relating perceptual judgments to learning. That perception is educable is clear from many "real life" instances. A butcher can heft a piece of meat and estimate its weight with great accuracy. The physicist Michelson (**198**) became so skilled at estimating the degree of distinctness of the fringes of light waves that these visibility ratings permitted him to compute the nature and shape of the spectrum lines. The "facial vision" of the blind has been shown to be attributable to their learning to perceive and respond to minute changes in the reflected sounds originating in their own actions (**26, 159, 203**). And there is William James's eloquent statement of the case. He said: "That 'practice makes perfect' is notorious in the field of motor accomplishments. But motor accomplishments depend in part on sensory discrimination. Billiard-playing, rifle-shooting, tight-rope dancing demand the most delicate appreciation of minute disparities of sensation, as well as the power to make accurately graduated muscular response thereto. In the purely sensorial field we have the well-known virtuosity displayed by the professional buyers and testers of various kinds of goods. One man will distinguish by taste between the upper and lower half of a bottle of old Madeira. Another will recognize, by feeling the flour in a barrel, whether the wheat was grown in Iowa or Tennessee. The blind deaf-mute, Laura Bridgman, had so improved her touch as to recognize, after a year's interval, the hand of a person who once had shaken hers; and her sister in misfortune, Julia Brace, is said to have been employed in the Hartford Asylum to sort the linen of its multitudinous inmates, after it came from the wash, by her wonderfully educated sense of smell" (**87**, p. 509).

During World War II a need developed in many areas for knowing the potential perceptual skill of a man in judging such aspects of the world as the size, distance, angle, and speed of obstacles, targets, and other

[1] This research was supported in part by the United States Air Force under Contract No. 33(038)-22373 by Human Resources Research Center, Perceptual and Motor Skills Research Laboratory, Lackland Air Force Base. Permission is granted for reproduction, translation, publication, use, and disposal in whole and in part by or for the United States Government. A more detailed discussion of some of the material included herein will be issued as a research bulletin by the Air Training Command.

[2] Dr. Dickens Waddell and Mrs. Elizabeth Lambert assisted the writer in collecting bibliographical material.

objects in the environment. Often the available level of skill left much to be desired and the question at once arose whether training could improve it. The kind of practice which will be most effective in increasing the skill of perceptual judgments is clearly a problem for experimental investigation. It is true that there has been a resurgence of interest in learning as an explanatory concept in perception, as witness Ames's revival of the Helmholtzian unconscious inference (**20, 86, 96**), Hebb's empirical and neurological theory (**69**), and the "new look" in perception, which stresses cultural relativism and individuality of perception, with past experience as a possible determiner of differences (**13**). Yet these new and very popular trends have not, to any impressive degree, resulted in a program of experimental research on learning in perception, that is, with experimental control of practice and experimental measurement of perception.

It is the purpose of the present review to bring together available facts which show the effects of training on perceptual judgments, with the hope that some specific questions can be answered. Can perception be rendered more acute by training, in the sense that the observer (*O*) comes to discriminate smaller differences in stimulation? Can perceptual judgments of single stimuli be brought into more precise correspondence with physical scales? Can the ability to recognize complex patterns of stimuli under conditions of dim light or short stimulus duration be improved? What are the most favorable conditions for perceptual learning? And how general is it? Can transfer be expected? These are all practical questions, both in the sense of having immediate applicability, and in the sense that the psychologist has the methods for answering them. The answers can be looked for in experiments which control and vary the frequency of practice and other factors such as kind and amount of reinforcement. The perceptual judgments of *O*, the dependent variable, can be obtained and measured by the techniques of psychophysics.

The studies chosen for report are limited by these questions to ones which deliberately manipulate practice in the experimental situation, or at least quantify practice which took place outside it.[3] This formulation of the problem of perceptual learning will be referred to as the problem of *improvement* in perception. The criterion of improvement will be defined in terms of veridical judgment, that is, evaluation of the judgment in terms of standards known to the experimenter (*E*) by virtue of the physical yardsticks available to him. If an *O* is being trained to estimate

[3] Other limitations should be noted. The effect of practice on judgments primarily of interest for reasons of physiological explanation has not been included; for instance, flicker fusion, though it may, apparently, show threshold changes as a function of practice (**207**). Experiments on animals are omitted, except in a few cases where they happen to suggest general principles, because it is too hard to disentangle the learning which is really due to perceptual practice and that which is due to learning the nature of the task and the response categories demanded by *E*. This latter factor, in human experiments, may play a role, but it is usually minimized by the preliminary instructions given *O*. Finally, it will be noted that very few experiments are reported in which *O* designated his judgment by a motor response other than a verbal one. This is not a systematically defensible omission, in itself, but unfortunately, the more complex the motor response required, the less clear it becomes whether any perceptual learning has taken place. All the "deprivation" experiments have been omitted—that is, experiments in which normal practice is prevented by temporary immobilization or sensory deprivation, but practice itself is not varied.

angles in a gunnery course, the measure of his learning is given by the deviation of his estimates from the true angle as defined by some measuring instrument such as a protractor. Improvement is arbitrarily defined, then, as closer, more precise, more immediate approximation of *O*'s judgment to the appropriate physical standard or measure. This definition means that some kinds of judgment, such as Rorschach responses, are excluded, since there is no way of scaling "rightness" or "wrongness" and plotting a learning curve. Highly personalized perception, while certainly affected by *O*'s past experience (**13**), is irrelevant for the problem as stated, owing to the absence of a criterion which permits measurement of learning.

Practice, for present purposes, will be defined as any controlled activity of *O* which involves repeated perception of the test stimuli or ones similar to them. This definition assumes attention on the part of *O*, but it deliberately omits any requirement of reinforcement, correction, or reward. The role of these factors will be examined in the light of the evidence. The facts which demonstrate improvement in perception will be summarized first, and then the factors which affect this learning, such as amount of practice and reinforcement, will be considered. Finally, transfer and retention of perceptual learning will be discussed, in so far as the facts permit.

Evidence of Improvement of Perceptual Judgments by Practice

The psychological literature of the past 70 years contains a vast amount of evidence that perceptual judgments can be improved. It will be summarized briefly, since the extensiveness of the literature does not permit inclusion of experimental details. Classification will be mainly by the operations used in measurement.

Acuity

The psychologist is tempted to think of acuity as a baseline which defines the structural limits of perceiving. Yet, acuity measures do vary with such factors as the kind of judgment required, and the lighting and nature of the test object. Possibly they vary also with practice in the judgment demanded. The effects of training have been investigated in the following areas.

Correction of visual anomalies. There have been numerous claims by optometrists that not only an improvement of acuity, but also the correction of astigmatism, eye-muscle imbalance, weak binocular fusion, nearsightedness, and other anomalies can be accomplished by prescribed training, usually involving stereoscopic targets (**17**, **23**, **102**, **131**). Woods (**200**), at the Wilmer Institute, reported results on 103 myopic patients tested before and after such training and concluded that expected normal variation accounted for most of the apparent improvement. Morgan (**127**) gave similar training to patients with seven types of anomaly, and concluded that the treatment must be varied depending on the source of the difficulty. Peppard (**133**) recommends the "Bates" method of exercise (practice in "central fixation," relaxation, the "long swing," etc.) for correction of visual defects. The validity of his claims has been discussed by Allbaugh and Miller (**1**) and by Lancaster (**101**). They are not supported by experiment.

Foveal visual acuity. On the other hand, positive evidence has been obtained, in an experimental situation, for the effect of practice on a typical

acuity judgment. Volkmann (**182**), in 1863, first reported such a change. Later Sanford (**148**), using letters as test objects, and Wilcox (**194**) and McFadden (**119**, **120**, **121**), using parallel bars, found large increases in acuity with practice. Bruce and Low (**12**), testing with Landolt rings, reported an increase in acuity after training with tachistoscopic presentation of aircraft photos.

Peripheral visual acuity. Dobrowolsky and Gaine (**31**) in 1876 reported an increase in peripheral visual acuity with daily practice sessions up to six weeks. Low (**114**, **115**) found that his *O*s could respond to stimulation of an area only 1/11 the size of that necessary before training, after 25 hours of practice with airplane silhouettes presented on various meridians of a perimeter. Test objects were Landolt rings of progressively decreasing sizes.

The two-point limen on the skin. Since the receptor surface of the skin is spread out like the retina, the techniques of investigating cutaneous sensitivity are analogous to methods of determining visual acuity. Titchener (**167**) and Luciani (**116**) both commented on the effectiveness of practice in lowering the two-point limen. Volkmann (**181**) found in 1858 that the distance at which two points were felt as double could be halved with practice. Dresslar (**33**), Solomons (**156**), Tawney (**162**), and Mukherjee (**128**) all obtained similar decisive results. Hoisington (**77**) contradicted this finding, but several factors, including the *O*'s previous level of practice, which was high, and the area chosen for stimulation, the forehead, where the two-point limen is already quite low (see **81**), could explain the lack of improvement.

Upper and Lower Thresholds

Like acuity, the upper and lower limits of sensitivity appear to be susceptible to the influence of practice. Titchener, comparing determination of RL's by two psychophysical methods, said, "The RL is a variable value: variable not only for different *O*'s, at different stages of practice, with different degrees of attention, but intrinsically variable" (**167**, p. 21). He noted the lowering of the intensive RL for pressure with practice, and persistence of the effect after a three-day interval. That practice must be allowed for in getting such a limen is clearly indicated in the methodology of psychophysics. Guilford, for instance, in discussing the method of measuring the lower limen for pitch of tones, gave sample data for its determination by the method of minimal changes in which the difference between the first and last ten series was "almost significant enough to suggest a lowering of the limen as if by a practice effect during the course of the experiment" (**65**, p. 121). Humes (**84**) found that practice raised the upper limen for tonal discrimination. Brown (**11**), in studying the absolute threshold for salt with human *O*s, thought the limen was not progressively lowered with practice, but that it fluctuated with the range of stimuli presented and with *O*'s attitude. But Harriman and MacLeod (**68**) demonstrated a very significant lowering of the salt threshold in rats as a result of practice under conditions of thirst motivation and electric shock reinforcement.

It has been shown by Verplanck, Collier, and Cotton (**176**) that even when *O*'s threshold is relatively stable on succeeding days, the responses themselves are dependent on previous responses; that is, runs of "yeses" and "noes" occur which are of greater length than expected on a hypothesis of independence. The same group of investigators found a

a shift downward of as much as .10 log units in the visual threshold after occasional reinforcement by *E* during threshold measurements (**177**).[4] Upper and lower thresholds and acuity are probably analogous to a physiological limit concept in learning, and do not necessarily imply a basic sensation in fixed correlation with a receptor structure. The very concept of a threshold is a statistical one; a lowered threshold as a result of practice is not paradoxical or inconsistent with any doctrine except the doctrine of fixed and immutable sensations automatically touched off by stimulation of a specific receptor.

Discrimination of Hue

Can hues not originally within *O*'s limits of discriminability become effective stimuli with training? During the war, a number of optometrists attempted to "train" hue discrimination in color-deficient *O*s and in some cases claimed success (**35, 36, 109**). It seems likely that improvement in these cases was due to increasing information about the tests themselves, which were usually some form of pseudo-isochromatic charts. Gallagher, Ludvigh, Martin, and Gallagher (**54**) found that training with such plates did not improve performance on a desaturation test, nor did training with the latter improve performance on pseudo-isochromatic tests. Chapanis (**22**) tested three "improved" cases after four years and found that pseudo-isochromatic charts could be read only where learned clues enabled *O* to call the correct responses. Learning correct responses for pseudo-isochromatic plates could be cued by brightness differences in the plates (**9, 45**).

[4] For another instance of lowering of an absolute threshold by training, see Blackwell, H. R. Psychological thresholds. Ann Arbor: *Univer. Mich. Engng Res. Inst. Bull.* No. 36, 1953.

Discrimination of Relative Differences

Can the discrimination of small differences between stimuli be improved by practice in comparing one with another? A relative discrimination will be taken as a judgment which consists of a comparison of the given stimulus with a designated preceding or adjacent stimulus (**183**), in contrast with an "absolute" estimation. In asking whether relative judgments can be improved, the distinction must be made between variable and constant errors—we want to know whether a finer just noticeable difference can be achieved with training. An *O* could correct a constant error in bisecting a Galton bar without necessarily lowering his variable error. On the other hand, DL's and constant errors could be lowered at the same time.

Pitch. The effect of training on relative discrimination of pitch has been investigated in 12 studies, only two of which found no improvement. The method of practice is undoubtedly a crucial factor here. Stanton and Koerth (**157, 158**) used no special practice, but gave the Seashore test before and after three years of general music study at the Eastman School. No general improvement in relative pitch discrimination resulted. Buffum (reported in **206**) found no improvement with mere repeated administration of the test situation. All the others obtained lowered thresholds, although Wright (**204, 205**) found improvement only in the six lowest initial scorers after five repetitions of the Seashore pitch discrimination test. Smith (reported in **206**), Whipple (**192**), Smith (**154**), Cameron (**18**), Wolner and Pyle (**197**), Seashore (**152**), Capurso (**21**), Connette (**24**), and Wyatt (**206**) obtained improvement in varying degrees, in some cases very impressive. Elaborate training methods caused

DL's to be halved or better. In one study, children who initially could not distinguish octaves could distinguish semitones or finer after training (**197**).

Weight. Brown (**10**), using the constant method with lifted weights, gave *O*s a large number of trials with correction, and found that judgments of larger differences showed irregular improvement ("lopping off of the coarser errors"), but no better discrimination of minimal differences. Urban (**171**) studied the effect of progressive practice in lifting weights, using a constant method, but no correction. The coefficient of precision (h) increased, and the interval of uncertainty grew smaller. Fernberger (**42**) repeated the experiment with similar results.

Visual space. The judgment of nearer-farther as a function of practice has apparently been studied only under conditions which eliminate many of the normal cues. Van Tuyl (**173**) eliminated all cues except accommodation and convergence and asked *O*s to judge which of two lights presented successively (in a dark room) was nearer. Only two of seven *O*s showed learning curves. Since blur would accompany change in either direction, it is hard to see how learning could occur, except by way of accompanying convergence cues. One *O*, who was myopic, "learned" to call the far points near and the near points far beyond a certain distance. Theories of space perception which assume that we see distance because we learn meanings for cues, such as changing accommodation, receive little support from this study. On the other hand, an experiment similar to Van Tuyl's by Peter (reported in **201**, p. 672) found that one *O*, helpless at first, learned to utilize cues of time and difficulty of focusing. The cues were inferential at first, but the depth impression, with practice, became immediate.

That relative judgment of two-dimensional length, or visual extent, can be improved with practice has been shown by Wolfe (**196**), Moers (**126**), Woodrow (**199**), Hamilton (**67**), Ueno (**170**), and Eagleson (**37**). Both CE and MV may be decreased in this type of judgment; whether either one or both types of improvement occur probably depends on whether correction is given. Welch (**189**) found that relative discrimination of form (varying along a continuum of squareness to tall rectangularity) was improved by practice in young children.

Spatial illusions provide an excellent situation for determining the effects of practice on otherwise compelling constant errors. Many experiments have investigated this question, with conflicting results. The conflict is probably due to lack of control of *O*'s attitude; *O* can always compensate deliberately for an illusion which he knows is present. Judgment of the Müller-Lyer illusion became more accurate with practice in experiments by Judd (**92**, **93**), Lewis (**110**), Seashore, Carter, Farnum, and Sies (**150**), Crosland, Taylor, and Newsom (**28**), and Köhler and Fishback (**98**, **99**). Several *E*s reported that the illusion diminished gradually and without conscious correction. Judd reported that "it comes to look differently than it did at first" (**92**, p. 29). Three of the *O*s of Köhler and Fishback developed a "negative illusion."

Other illusions have yielded equivocal results with practice. Williams (**195**) found no change in the horizontal-vertical illusion, but *O*s "tried" not to let their knowledge of the illusion affect their judgments. Ritter

(**143**) studied judgments of length or lines inclined at various angles from the horizontal. The illusion tended to decrease if the line originally overestimated was varied in length during practice. Seashore *et al.* (**150**) obtained ambiguous results with cylinders in vertical and horizontal positions, also with a *T* illusion, and in estimating the distance between two circles. Cameron and Steele (**19**) found gradual disappearance of the Poggendorf illusion with long practice. Judd and Courten (**95**) found a decline in the Zöllner illusion with practice, after an initial rise. In these experiments, most investigators were convinced that the decrease in CE occurred without any conscious attempt at correction, but *O*'s attitude was not deliberately varied in both directions in any study.

Absolute Estimation and Rating

That absolute judgments of stimulus magnitude and quality may be improved with practice has long been recognized. The method of "single stimuli" is admirably adapted to the study of "object judgments" or ratings, and the literature in this field has for a long time taken account of *O*'s experience with the test stimuli. Hollingworth's concept of "central tendency of judgment" (**78**) and Helson's "adaptation level" (**71**) reflect such an interest, as does the literature on anchoring (**122, 145, 184**). There is a correspondence between the actual range of stimuli presented and *O*'s conception of the stimuli as a series; this conceptual series tends to persist, but is responsive to change in the range of stimuli presented (**130, 191**). The category thresholds and the midpoint of the scale will reflect such a change. Johnson (**88, 89**) has incorporated these facts in a theory of how a subjective scale is learned. The units (categories) of the scale may be familiar and conventional ones, such as inches or minutes, or they may be arbitrarily chosen by *E*; the number of response categories may vary, and they may be adjectives, such as "heavy" or "light," or numerical values. He makes the following assumptions: that the scale is related to physical values of the stimulus objects presented; that each judgment of a stimulus object constitutes a unit of practice; that *O* makes his judgment with reference to the midpoint of the scale; that any given stimulus generalizes up and down the scale in linear fashion; that the midpoint of the scale is the arithmetic (in some cases, geometric) mean of the central effects of the stimuli upon which the scale was founded. These assumptions are nearly all accepted facts of judgment; the novelty lies in Johnson's attempt to relate scale development to learning theory by counting units of practice. He combines these hypotheses in equations yielding predictions which he has checked with weights (**88**) and with tones differing in pitch (**90**). His results show, in general, good agreement between predicted and obtained limens. A further experiment with pitches (**91**) investigated the change in the scale with introduction of new stimuli offered for judgment. The midpoint of the scale was found to shift, as predicted, and furthermore, the shift was retarded relative to the frequency of preshift trials with the first series. Tresselt (**168**) anticipated the latter finding in an experiment with weights in which she found that as practice with the first scale increased, the scale of judgment tended to shift more slowly to its new position. The "range" effect can be related to *improvement*,

since the subjective midpoint turns out to be anchored to the physical midpoint of the series used. If *O* has experienced the entire range of relevant stimulation (all that the environment provides), his midpoint would be realistically anchored.

The training procedure most often followed in the studies providing evidence of improvement in absolute estimation is quite similar to a paired-associates procedure. The stimuli to be judged are paired with labels indicating the appropriate magnitude on the physical scale chosen. The difference between this learning situation and ordinary identification learning (learning names of people, things, nonsense forms, etc.) is that the range of stimuli may be placed along a dimension or continuum; furthermore, the responses which *O* is to learn may be scaled and placed in a consistent relationship or correlation with the stimulus continuum. The term "scale training" might be used to indicate the procedure. The following experiments have demonstrated that improved judgments result from such training.

Pitch. According to Petran, "Judgments of absolute pitch may be defined as judgments based on associations learned between more or less narrowly limited ranges of the pitch series and the terms of any unequivocal nomenclature, these judgments being without reference to or aid from any tone or tones recently heard which have been given as a standard or attended to in any degree as being of a certain pitch or familiar pitch position" (**134**, p. 12). The *O* starts with a large number of very similar stimuli and gradually achieves identification of each tone by its appropriate name. But precision, even for a highly trained musician, is never complete. Petran showed that there is a narrow range of equivalent frequencies, rather than a single numerical frequency which is adequate to elicit the judgment of a given note.

Four studies have dealt specifically with the effects of practice. Meyer (**124**) used progressive practice with a total of 39 notes, a whole tone apart. Notes were called by vibration rates read from a chart. Four months of practice produced improvement, which was largely lost after several years of no practice. Accuracy was inversely related to the number of tones practiced. Gough (**63**) used all 88 keyboard notes on the piano; *O* was instructed to name each note and its octave from a keyboard model. Varying procedures were used. Average error for all *O*s decreased from 5.5 to 4.5 semitones. Those who practiced longer improved more. Retention was good after a year. Practice on a single note improved recognition of it. Fewest errors occurred in the middle octave, perhaps because of more frequent practice of this octave. Mull (**129**) trained *O*s on only one note and tested them by requiring recognition of this note among a group of nine tones. Average error decreased from 2.85 to .33 semitones and finally to .29 semitones when the difference between the tones was narrowed. Wedell (**187**) had *O*s identify tones by vibration rate, represented at appropriate points on a chart. Average error in DL's was cut in half or more by training. Greatest error occurred in the middle of the scale. There was no tendency to constant error. With progressive practice, all *O*s learned to identify nine tones without error. When the series was longer (13, 17, and 25 tones), no one mastered it in the number of trials given, but a learning trend is apparent.

Weight. Fernberger (**43**) had *O*s judge weights on an absolute three-category scale. Practice had the same effect as in a relative series; precision increased, the interval of uncertainty decreased, and the *p.s.e.* increased. Practice also resulted in a reduction in reaction time (Fernberger and Irwin, **44**). Thorndike and Woodworth (**164**) gave *O*s training in estimating weights in grams and found improvement.

Visual space: area and extent. The classic experiments of Thorndike and Woodworth (**164**) show that the estimation of the areal size of different geometrical shapes, such as rectangles, circles, and triangles, can be improved. In another experiment, *O* judged lines of different length, and improvement was again evident. One gets "more accurate mental standards and more delicacy in judging different magnitudes by them. In the case of estimations of magnitudes in terms of unfamiliar standards such as grams or centimeters, the acquisition of the mere idea of what a gram or centimeter is, makes a tremendous difference in all judgments" (**164**, p. 394). Thorndike (**163**) later did further experiments on visual estimation of length in centimeters and area in square inches, with marked improvement resulting.

Van Voorhis (**174**) conducted a large-scale experiment on improvement of "space perception ability," the criterion being score on the Cards and Figures section of the Thurstone test for Primary Mental Abilities. Training included estimation of linear extent, angles, and areas, various visualizing exercises, three-dimensional tick-tack-toe, and practice with a stereoscope. Scores improved significantly relative to a control group.

Postman and Page (**138**) had *O*s judge either height or width of rectangles by a variant of the method of single stimuli. Precision increased during the series and DL's decreased. Retroactive inhibition resulted from interpolation of the alternative judgment.

Visual space: angle. In an experiment done in an Air Force Gunnery Training program (**76**), gunners were trained in estimating the "aspect angle" of a plane. This was the angle between the line from gunner to target and the line extending through the longitudinal axis of the target aircraft. A training device was used with a model fighter set at a simulated range of 400 yards and variously inclined to the line of sight. Judgment was improved by training.

Visual space: depth and distance. Judgments of stereoscopic depth improved with practice in two experiments where stereoscopic range finders were used (**79, 209**). Range-estimation trainers which require a judgment of horizontal extent or of proportion of the reticle filled have also been studied. An experiment of this type with the RCAF range-estimation trainer found marked improvement as a result of even a small number of training trials (**76**, pp. 148 ff.).

Studies of the effect of training on unaided visual-range estimation have yielded positive results (**80, 180, 208, 210, 211**). Two large-scale studies used aerial targets viewed against an expanse of sky (**80, 179**). The *O*s estimated in yards; range of targets varied, in one case up to 8,000 yards. The *O*s improved with corrected practice in both cases, and constant errors were generally reduced. A comparison of various methods of training for estimating a fixed opening range (**144, 178, 185**) suggested that training on the firing line was more effec-

tive than special trainers, and that unaided vision, after training, was more accurate than stadiametric estimation. The Princeton Fire Control project (**210**) utilized existing targets, such as telephone poles viewed over a ground surface, at distances varying from 235 to 5,940 yards. Training resulted in improved accuracy of estimates, and also in a reduction of variable error for both the group and individual *O*s. Constant errors shifted irregularly for different targets.

Visual speed. Speed of motion of airplanes of various types, flying different courses at different speeds, was estimated by anti-aircraft officers in an experiment by Biel and Brown (**6**). Corrected practice had the effect of raising estimates all through the speed range, causing slow speeds to be overestimated more than before training, so that improvement was limited to high speeds. Group *SD*'s decreased, but consistency of individual estimates was not improved by training.

Visual form. A study was made by Gilliland (**61**) of the effects of practice on grading handwriting. Unaided practice in grading samples on a 1–100 scale was followed by practice with a "true" scale for reference and with knowledge of results. Errors were greatly reduced.

Brightness and hue. Though no direct study of training has been made for absolute estimation of these qualities, two studies are suggestive. Lehmann (**108**) showed *O*s a series of grays arranged in a scale from black to white. Then each member was exposed singly and *O* was required to judge its order in the series. A five-member series was judged with great accuracy (96.7%), but with longer series accuracy dropped. Lehmann thought that the absolute scale was limited by *O*'s ability to label the stimulus presented, and that *O* had labels readily available for only a five-member scale (black, dark gray, medium gray, light gray, white). Halsey and Chapanis (**66**) found that the number of absolutely identifiable spectral hues was limited to 10 or 12, even after practice, but they expected that certain errors might be eliminated with further practice.

Kinesthetic extent. Thorndike (**163**) showed that *O*s improved in their ability to draw a line of a specified length, such as three inches, while blindfolded, under certain conditions of practice. The result has been corroborated (**169**).

Pressure. Thorndike (**163**) also reported experiments on gauging pressure of a specified strength on a dynamometer. Practice followed by "right" or "wrong" resulted in improvement.

Recognition of Patterned Stimuli under Impoverished Conditions of Stimulation

When patterned stimulus material is presented under conditions deliberately made far from optimal, such as very low illumination, brief exposure, presentation to parts of the sensory surface where receptors are few, reduced size, etc., the conditions are referred to as "impoverished" (**58**, p. 166). Test objects which might be easily identified under favorable conditions may not be recognized. A threshold measure can be calculated in terms of the amount of light, duration of exposure, etc., required for accurate identification. Many experiments have investigated the effects of practice in perceiving under these conditions.

Peripheral presentation. A series of experiments on "peripheral retinal

learning" were conducted by Franz and various collaborators (**48**, **49**, **50**, **51**), all of which showed improvement in accuracy of recognition of geometrical forms or words with repetition of them. Three figures were exposed at once, for .1 sec., displaced respectively to the right, left, and above a fixation dot. Drury (**34**) also studied the effect of repetition on peripheral perception, and found that drawings of the diagrams presented became stabilized, though not necessarily more veridical. Henle (**73**) tested accuracy of peripheral perception as a function of frequency of experience with the figure *prior* to the test situation, and found that past frequency facilitated perception. Postman, Bruner, and Walk (**137**) confirmed the result.

Low illumination. Seward (**153**) studied the effects of practice on identification of letters presented dimly on a ground-glass screen for 1.5 sec. each. Practice (without correction) resulted in gradual, continuous improvement of all *O*s. Bevan and Zener (**5**) found thresholds for recognition of nonsense forms by raising illumination to a liminal level and showed that both general practice effect and specific familiarity affected the threshold, the specific practice effect being greater and proportional to frequency of pretest exposure.

Recognition of ships or obstacles by night lookouts is a practical problem similar to the studies just mentioned. Training programs for night lookouts were established by the armed forces during World War II. Training usually took the form of practice in looking at models and diagrams displayed in dim light in a viewing box (**172**, **188**).

Brief presentation. Repeated presentation of the same stimulus material in a tachistoscope has been the traditional approach to what is often called the "development" of perception. Older studies were primarily concerned with the stages of development, but it can also be shown that repeated brief presentation may result eventually in accurate perception. Fehrer (**41**) presented line figures successively in short exposures. Stages occurred, but the figures were eventually perceived correctly. Irwin has shown that limens for accurate recognition of barely perceptible patterns presented for .18 sec. are reduced with practice (**85**). Postman and Bruner (**136**) investigated the effect of different kinds of preliminary training on the threshold for perception of a gap in the outline of a circle. Preliminary exposure of open circles reduced the threshold for perception of an opening. But previous viewing of closed circles also reduced the threshold relative to a control group, though not as much.

It is possible to study the effect of practice on the *amount* of material which can be reported after tachistoscopic exposure. Historically, this is the problem of the "span of apprehension" as a function of practice. Tinker (**165**) reviewed the pertinent experiments in 1929 and concluded that practice increased the range of apprehension, but that the effects were very specific. A later experiment by Weber (**186**) corroborated his conclusion. Educators took up the same problem under the heading of "flash training," and their work will be considered under transfer, since transfer to reading skill was the question at stake. But the results uniformly showed that training with a tachistoscope has a significant effect on performance in the training situation (**55**, **57**, **142**).

The judgment of number as a func-

tion of practice has been studied in three experiments. Taubman (**161**) found that practice in estimating the number of tones presented at rates of .1, .07, or .056 sec. resulted in improvement, though overcompensation for a constant error sometimes occurred. Saltzman and Garner (**147**) had *O*s estimate the number of circles in a group of concentric circles exposed for .5 sec. With practice, reports became more accurate and reaction time decreased. Knowledge of the range of the number of circles improved accuracy immediately. Minturn and Reese (**125**) studied the effect of differential reinforcement on estimation of number of dots presented visually for .2 sec. Error decreased, though there was some tendency to overcompensate for constant errors.

Relative familiarity with material gained prior to the tachistoscopic viewing also affects differentially the accuracy of perception. Vernon (**175**, pp. 123 ff.) showed that unfamiliar words were misread when presented tachistoscopically 52 times each, compared with 2.8 times for more familiar words. Leeper (**107**) familiarized *O*s with Street figures (incomplete pictures) by preliminary exposure, naming, demonstration by *E*, etc. In a later tachistoscopic test (.01 sec. exposure), these *O*s named the figures more accurately than a control group. Howes and Solomon (**83**) found a strong inverse relationship between relative word frequency (frequency of occurrence in the English language) and visual duration threshold. In the same experiment, the effects of tachistoscopic training are also apparent. The frequency-duration threshold relationship has been corroborated by Postman and Schneider (**139**) and by McGinnies, Comer, and Lacey (**123**). Recency of word usage has also been correlated with duration threshold (**140**). Differential word frequencies which were established experimentally have also been shown to be correlated with duration threshold (**155**).

Noise and distortion. The problem of communication against a background of noise is in some ways comparable to viewing under impoverished conditions. When listeners were trained for hearing over an interphone system (**7**), greatest gains were produced by practice under test conditions with the actual words used. But practice with the words under other conditions helped. Howes (**82**) analyzed data collected by Mason and Garrison (**118**) on intelligibility of spoken messages heard under conditions of noise and found a significant correlation between frequency of correct transcription and average frequency (probability of occurrence) of the words used. That improvement in understanding of masked or distorted speech occurs with practice over and above increasing familiarity with the words spoken has been shown by Licklider and Pollack (**111**) and by Egan (**38**).

Factors Influencing Improvement

Amount of Practice

That improvement in perceptual judgments occurs with practice is evident. What is the function which describes the relation between amount of practice and degree of improvement? Merely to say that there is an effect does not satisfy the psychologist interested in learning; he wants to see the learning curve. Undoubtedly, there will be many curves, not one, depending on the operations measured, units chosen,

and other factors. And unfortunately, few of the experiments have measured the effects of practice at enough points along the baseline to describe a function. That frequency is a significant variable can be concluded, however, from a number of studies (**5**, **28**, **41**, **63**, **83**, **153**, **168**, and others), and that early practice is more effective than later stages is probable (Fernberger, **42**).

Learning curves have, it is true, been plotted in a few studies. Woodworth (**201**, p. 182) plotted data obtained by Volkmann (**18**) for the effect of practice on the two-point threshold. The curve of errors falls gradually with a negative acceleration. Seward (**153**, p. 33) presents individual curves for the number of letters correctly reported under conditions of dim illumination and brief exposure as a function of days of practice. All the curves show a very gradual and continuous rise with no marked acceleration. Howes and Solomon (**83**, p. 407) plotted duration thresholds showing the effect of amount of practice on tachistoscopic viewing. About three-fourths of the practice effect was accomplished in one-fourth of the experiment, but the drop is continuous and is still apparent at the final (60th) threshold measured. Bevan and Zener (**5**, p. 439) plotted the curve for the intensity threshold required to perceive meaningless figures as a function of pretest frequency of viewing. The decrease in intensity is progressive and negatively accelerated.

In contrast to these gradual and continuous functions, Minturn and Reese (**125**, pp. 222 ff.) found that reduction in error of judgments of numerosity occurred very suddenly. In this case, *O*s were able to compensate immediately for constant errors which had been consistently present in pretraining judgments. But correction of a constant error is not always immediate, for Judd (**93**) found that 24 days' practice with the Müller-Lyer illusion yielded a gradually sloping curve (average error dropped slowly from 17.3 to 1.7 mm.). The *O* was said to know the effects of his practice.

Reinforcement

Ample evidence exists to establish the proposition that improvement in perceptual judgments is a function of frequency of practice. Does this mean that frequency of repetition, pure and simple, is a sufficient condition for perceptual learning? Or must practice be reinforced in some way?

Perceptual learning without reinforcement. Evidence of perceptual learning without any *apparent* reinforcement does exist in experiments where *E* has not deliberately introduced correction or reward. Studies of the two-point limen in at least four separate experiments (**33**, **128**, **162**, **181**) showed learning without reinforcement by *E*. However, a method of limits, as is customarily employed in these experiments, provides *O* with a clear example of both "twoness" and "oneness." Perhaps he can check himself, to some extent, by these anchoring experiences. The limen did not decrease when a constant method was used (**77**). In experiments on absolute judgment by the method of single stimuli, merely "getting the range" of a set of stimuli can affect *O*'s judgments (**89**, **130**, **191**) and stabilize them in predictable ways. No reinforcement is given by *E*, though *O* may predict and check for himself as his concept of the range is anchored by experience with the stimuli. Woodworth took the position that there is a direct perceptual motive "to see clearly, to

hear distinctly—to make out what it is one is seeing or hearing" (**201**, p. 123). Such a motive would lead *O* to reinforce himself insofar as possible. But several experiments appear to present almost no opportunity for even self-reinforcement. Relative comparisons of lifted weights (**42**, **171**), for instance, improved with practice, although no knowledge of results was given. Successive presentations with a tachistoscope (**41**), and practice in viewing letters under dim illumination (**153**), resulted finally in accurate identification of stimuli. Reinforcement could have played very little role in these cases.

It is possible that judgments become more consistent in the absence of external reinforcement, without becoming more veridical. In Drury's experiment (**34**) on repeated peripheral presentation of patterned stimuli, all *O*s become more consistent in their drawings of the stimuli, but not necessarily veridical. When stimulation is ambiguous, *O*'s perception is apt to become schematized and stabilized, as Bartlett (**4**) showed, but veridicality probably depends on an external check. Seashore and Bavelas (**151**), using Thorndike's data (**163**), showed that repeated practice in drawing a line of a given length, without any correction by *E*, resulted in *O*'s closer and more consistent approximation to his own standard. They thought this stabilization was due to *O*'s *presumed* knowledge of results, however faulty.

If Woodworth's theory of perceptual reinforcement is correct, *O*'s perceptual judgments of the spatial world around him should tend toward veridicality, rather than internal standards, because locomotion in this world could provide the external checks needed.

Effect of reinforcement. Although the possibility must be admitted that perceptual learning may occur without correction or reward, all those experiments which provide a contrast of practice with and without knowledge of results show a clear superiority of the condition with knowledge. Thorndike (**163**) concentrated on this problem and produced evidence with many types of absolute judgment, such as visual length and area, kinesthetic length, and pressure to show the necessity of knowledge of results for improving these estimates. The *E* always said "right" or "wrong" in correcting *O*. Absolute judgments of number, practiced with and without differential reinforcement, have also been shown to improve more with reinforcement (**125**, **161**), though Taubman (**161**) found some improvement without it. Even the two-point limen was lowered more effectively when *E* gave correction. Solomons (**156**) gave stimulation with one and with two points in irregular alternation. An *O* who was corrected lowered his limen, but another *O* was uncorrected and showed no change. This *O* did improve, however, when *E* later introduced correction.

Relative judgments, likewise, benefit by correction. Improvement of judgments of relative pitch was found in every case where *E* provided knowledge of results (**21**, **24**, **152**, **197**, **206**), but little or no gain occurred with mere repetition (**204**, **205**, Smith and Buffum in **206**). In comparisons of length of line, some improvement has been found to result from mere repeated practice (**126**, **196**), but knowledge of results increased the effect. Moers (**126**) found that practice without knowledge decreased the MV, but actually enhanced the CE. Practice with knowledge reduced both.

The kind and amount of reinforce-

ment which will be most effective has received some attention. Probably improvement increases with the *amount of information* given by *E* in the correction. Trowbridge and Cason (**169**) repeated Thorndike's line-drawing experiment adding a condition in which *O* was told the amount and direction of his error. This procedure was far superior to "right" or "wrong." A similar comparison was made in an experiment by Franz and Morgan (**50**) on learning to identify peripherally presented forms. In three experimental conditions, *O* either paid attention to the forms but was not asked to report on them during practice, or he drew them as practice progressed and was told right or wrong, or he had a recognition test in which he chose the form just presented from the total group and was corrected. Learning varied as expected, with most learned in the last-mentioned condition, and least in the first. Hamilton (**67**), in an experiment with the Galton bar, compared conditions with (*a*) no knowledge of results, (*b*) "punishment" (a bell rang if *O* increased his error over a previously set standard), (*c*) "reward" (bell for reduction of error), (*d*) punishment plus a guess as to direction of error, (*e*) punishment plus knowledge given *O* of direction of error, and (*f*) knowledge given *O* of direction of error, but no bell. Improvement occurred for all conditions but the first. Improvement was least for knowledge without "reward" or "punishment." Knowledge in addition to "punishment" did not help. Hamilton thought the bell functioned as an incentive, which raises the question of the nature of the reinforcement in these studies. Is it merely informational, or is there an affective component?

Informational vs. affective reinforcement. In the studies so far reviewed, reinforcement has been generally synonymous with correction—knowledge given *O* by *E*, or knowledge gained by *O* from his own performance. A good example of the latter would be pitch training in which *O* sings the tones to be compared. In one case (**206**), a stroboscopic technique permitted *O* to see whether the tone he produced was accurate and to correct it when it drifted, thus providing a maximum amount of feedback, or information from *O*'s performance. If the informational aspect of reinforcement is the critical factor, it might be inferred that giving the information by an anchoring technique should be as effective as correction following judgment. Anchoring was effective in bringing about improvement in a few studies (**125**, **138**, **147**), but no systematic comparison with correction has been made.

The term "informational" may be misleading, for it is possible for *E* to reinforce a wrong judgment. In several experiments, *E* has applied reinforcement in such a way as to produce perceptual illusions (**16**, **39**, **177**). The *O*'s judgments in these cases tended to change in a predictable direction, but were in no sense "improved." Affectional reinforcement which gave no information relevant to the perceptual judgment measured was used by Lambert, Solomon, and Watson (**100**). Children associated poker chips with a candy reward in a two-stage token-type task. When the children reproduced the size of the token by means of a variable circle, it was made larger after the token-candy sequence, and decreased again following an extinction series. Why overestimation is produced by this technique is not clear, but at any rate a constant error is enhanced.

Whether this is learning in the same sense as the examples cited in the section on improvement is a disputable question. It seems safe to conclude that reinforcement by external correction or check is a very significant, if not an essential, variable for improvement in perceptual judgments.

Distribution of Practice

Only one study of perceptual learning, Lewis' on the Müller-Lyer illusion (**110**), has actually varied this parameter. Distributed practice (on alternate days) accelerated the rate of decrease of the illusion. Several studies have found, however, that periodic reinforcement is valuable for keeping the judgment at its peak of improvement. Evans (**40**) trained micrometer readers by having *O*s measure gage blocks and then read the size marked on them, noticing the size and direction of the error. When practice was continuous, there was a steady decrease in errors, but periodic practice with knowledge was necessary to maintain this improvement. Horowitz and Kappauf (**80**) trained *O*s in absolute estimation of distance. After 60 days of no practice there was an increase in average error, but a short training period restored skill. Periodic reinforcement was found necessary, also, to maintain skill with certain range-finding instruments (**79**).

Sequence of Practice

What is the best arrangement of material to be discriminated, if practice is to be most efficient? If the stimulus material can be scaled along a dimension, should practice be ordered in some relevant way? Common sense suggests that relative discriminations might be aided by beginning with points far apart on the scale and progressing toward closer and closer stimulus pairs. Animal experiments have in many cases borne out this hypothesis. Pavlov (**132**) used this method in establishing difficult discriminations in dogs. Köhler (reported in Koffka, **97**), in a color discrimination study with apes, found that in a series of hues lying between red and blue, ABCDE, two neighboring ones were not originally discriminable (e.g., BC). But if the animal was given practice in discriminating a wider interval (e.g., BD), the difficult discrimination could be learned. Lashley (**103**) found similar transfer to a brightness discrimination; rats trained first with black and white transferred easily to a pair of grays. Lawrence (**106**) trained rats to discriminate shades of gray with three different methods: (*a*) all practice (80 trials) given on the same two middle grays to be finally discriminated; (*b*) 30 trials on a black-white discrimination and the rest on the two grays; (*c*) progressive training from black-white through a transition series to the two grays. Efficiency of learning was in reverse order for the three groups. Harriman and MacLeod (**68**) also successfully used progressive reduction to bring about lowering of the salt threshold in rats. That the *progressive* narrowing of the difference is important seems clear from a study by Grether and Wolfle (**64**), who varied pairs of brightnesses presented for discrimination by rats, but always kept the ratio of light-dark the same. Varied stimulation did not increase efficiency of learning to make a light-dark discrimination; neither did it lower it until the number of different pairs was increased to four.

But with human *O*s, when preliminary instructions can make clear the dimension to be discriminated, will a progressive method have any value? It is possible that the method

is facilitative in animal experiments simply because it helps the animal to discriminate the relevant dimension of difference. Progressive training may serve some other function also, however, if it makes practice more effective with human *O*s. With relative discrimination of weights, Urban (**171**) used progressive practice and found lowered DL's. In the relative discrimination of pitch, progressive practice was a valuable technique (**152**). Wedell (**187**), in training absolute pitch, started with five widely spaced pitches, and gradually interpolated more in the intervals between them. These cases have not provided statistical proof of the superiority of progressively ordered training; nevertheless they suggest its value. If the aim of training is to establish a conceptual scale which is realistically anchored and well differentiated (in absolute estimation of spatial dimensions, for instance), ordered practice may be the best procedure.

Role of Names and Labels

William James, in his discussion of learning fine discriminations, suggested two ways in which he thought such learning might take place. One was the progressively ordered practice just discussed; the other was learning distinctive associates to originally barely discriminable pairs of stimuli. "First, the *terms* whose differences come to be felt contract disparate associates and these help to drag them apart . . . . The effect of practice in increasing discrimination must then, in part, be due to the reinforcing effect, upon an original slight difference between the terms, of additional differences between the diverse associates which they severally affect" (**87**, pp. 510 ff.). A similar hypothesis is expressed in Dollard and Miller (**32**). Evidence in its favor exists in the studies of relative pitch discrimination. Capurso (**21**) had *O*s learn "mood words" for intervals, and finally substituted the technical name for the mood word; Smith (**154**) used various associates, including adjectives (e.g. "dull," "heavy"), motor responses, and visual localization of tones in space. Three *E*s (**18, 197, 206**) had *O*s sing tones or intervals, thus adding kinesthetic feedback to the auditory stimulation. Learning was very effective, as exhibited by lowered DL's, in all these studies. However, they were not designed to isolate the effect of the verbal or motor response, so their role is not proven.

No truly critical experiment yet exists to test James's hypothesis with psychophysical measurements of sensitivity before and after the training. There are a few experiments now available (**3, 53, 104, 105, 146**) in which preliminary learning of differential responses for a range of complex stimuli has been shown to transfer to a second learning task, which involved different responses but the same stimuli, in such a way as to lower errors of generalization in the new task. But this may involve no shift in limens. That is, it is not clear whether the actual perception of differences between the stimuli has been affected. An experiment by Arnoult (**2**) is more relevant, but negative. He gave *O*s training in learning verbal associates (numbers) to nonsense forms. Then *O* made discriminative responses to the forms by pressing an appropriate key. Accuracy and latency measures showed no advantage over a control group which did no preliminary learning.

Transfer

Whether perceptual learning will transfer to new situations is a question of practical importance. Train-

ing aids, for example, must be evaluated for transfer from the trainer to the real situation, for trainers sometimes teach *O* to respond in terms of cues which are specific to the training device and irrelevant to the operation actually required. For instance, Viteles and others (**144, 178**) studied improvement in range estimation as a result of training on the Mirror Range Estimation Trainer and found it far less effective than unaided training on the firing line, owing in part to *O*'s tendency to respond to stimuli peculiar to the trainer. The doctrine of "formal discipline" in education is another practical case. It was first attacked experimentally in Thorndike and Woodworth's classic studies of perceptual learning and transfer (**164**). The *O* was given practice in estimating the area of rectangles; he was then given tests including rectangles and other figures. Transfer did occur, but while all six *O*s reduced their errors in the case of identical figures and areas, only five reduced them for the same shape but different areas, and only four in the case of different shapes. Similar results were obtained throughout: in judging the extent of lines of different length, improvement was greatest with the lines actually used in the training series, though there was some improvement with lines of other lengths. As every one knows, they stressed the importance of "identical elements" in transfer.

These results, however widely accepted, did not lay the ghost of "formal discipline," for there continued to be claims that the "power of attention" could be trained and that perceptual "skill" could be increased by practice in tachistoscopic viewing. Whipple (**193**) and Foster (**46**) investigated these claims and agreed that practice effects were specific and that transfer occurred only in proportion to similarity of the tasks. Weber (**186**) found no transfer from tachistoscopic training with letters to other material, and concluded there was no "G factor" of perceptual skill.

The interest of educators in "flash training" as an aid to reading skill has a similar background. Actual experiments including reading tests before and after tachistoscopic training and a suitable control group are rare; and sometimes other changes, for instance in motivation, are introduced at the same time, thus confounding the variables. Studies which found improvement (**27, 117, 149**) are often of this type, and the criticism has been made by Vernon (**175**) and by Henry and Lauer (**74**). Gates (**55**), in a well-conducted study, found that control groups which were "reading-trained" were superior in reading to experimental groups which were "flash-trained." Other studies bearing on the problem are those of Sutherland (**160**), Freeburne (**52**), Glock (**62**), and Renshaw (**142**). None demonstrates conclusively that flashmeter training or eye-movement training is superior to other methods of training in reading.

Renshaw (**142**) has emphasized the transferability of tachistoscopic training, not only for reading but for such activities as plane recognition, on the assumption that it trains a general "perceptual skill" which he compares with motor skills. He reported that Navy preflight trainees improved their skill at plane recognition by tachistoscopic training with digits and slides containing varied numbers of planes to count ("counter training"). But no data were presented for a control group without tachistoscopic training. The experiment was repeated with an appropriate control group by the AAF Psychological Test Film Unit (**57**). Tests

for recognition proficiency showed no advantage for the group which received tachistoscopic training, although their performance with the digit and counter slides improved. A somewhat similar experiment was conducted in an Air Force Gunnery Training program (**76**). The *O*s were given practice in observing, or observing and drawing patterns exposed on a large screen for .05 sec. Pre- and posttests on five criteria gave little evidence of transfer. A test of speed of plane identification resulted in a better score for a control group. It appears likely, in view of all the evidence, that perceptual learning with the tachistoscope transfers only insofar as the test and training tasks are similar.

Lindsley (**112, 113**) used a "flash-reading trainer" for radar operators with some success. The trainer simulated the presentation of the actual PPI type radar-scope. Signal blips were flashed briefly (2.5 sec.) on a screen marked with range rings and bearing lines or with a rectangular grid. Different problems (e.g., reading the location of a blip or counting blips) were given during practice. Improvement in accuracy of readings increased with training, and correlations showed a significant relationship between trainer proficiency and actual proficiency in operating the PPI scope. In this experiment, the trainer task was made as similar as possible to the operation to be performed.

Other types of transfer of perceptual learning have been investigated with positive results. Coover and Angell (**25**) thought that general habituation to experimental conditions was responsible for transfer. Their *O*s were tested for discrimination of shades of gray; then they were given training in discrimination of sound intensities for 17 days, and finally were retested on discrimination of the grays. Gilbert and Fracker (**60**) found that discriminative reaction time showed a similar transfer from auditory to visual discrimination.

Transfer of a general principle was demonstrated by Judd's (**94**) study of dart throwing at an underwater target. Knowledge of the general principle of refraction had a transfer effect when the spatial cues were changed. Hendrickson and Schroeder (**72**) repeated the experiment with an air gun; they minimized the motor control required, and found both original performance and performance on the changed problem facilitated by knowledge. Minturn and Reese found increased accuracy of estimation for dot patterns as yet unpracticed, following correction with a similar pattern (**125**). Here *O* could verbalize a tendency to constant error and apply it throughout the stimulus dimension.

Bilateral transfer and transfer to other areas of a sensory surface have been studied with the two-point limen and with peripheral recognition of form. Practice which lowered the two-point limen showed a similar lowering of the limen on corresponding symmetrical areas of the skin (**33, 128, 181**). Transfer to adjacent areas of the homolateral side was obtained by Mukherjee (**128**) but not by Volkmann (**181**). Franz and his collaborators (**48, 49, 50**) found that practice which increased accuracy of recognition of forms presented peripherally transferred to the corresponding retinal area of the opposite eye. Homolateral transfer to untrained retinal areas also occurred (**50**) but to a lesser degree. The areas tested in the latter case were farther out on the periphery than the trained area.

An experiment by Fracker (**47**)

suggests that transfer may also be mediated by a learned "conceptual schema." His *O*s were asked to reproduce the order in which four tones, differing in intensity, were presented to them. The tones were labelled 1, 2, 3, and 4, according to intensity, and were presented in different patterns. After long practice, the *O*s were asked to reproduce orders for other series—four grays, nine tones of different intensity, nine grays, and four tones of different pitch. Highest transfer occurred for the four grays, and different degrees for the others. Introspections indicated that *O* had learned to use a spatial schema for ordering intensities, which could be carried over to the four grays.

How transfer operates when stimuli are shifted along a continuum is a question of theoretical interest since it is related to the problem of transposition and to the question of whether a response to a given stimulus quantity is originally specific or is only relative. Cameron (**18**) had *O*s practice singing one standard tone for several months. Relative discrimination was tested, and this was improved by the practice only when the practiced standard was one of the pair to be discriminated. The outcome suggests that training in absolute identification of a given tone can reduce stimulus generalization between that particular point and neighboring ones but does not increase specificity for other points in the scale. Wedell (**187**) studied the training of absolute pitch, in order to discover whether *O* learned to recognize specific individual notes, or learned the position of notes with respect to a whole scale. If the latter occurs, Wedell thought he should obtain some ability to identify notes never practiced, provided they are within the range of notes practiced. He gave practice with 25 notes, equidistant in pitch, which had to be identified by vibration rate. The *O* had before him a chart representing the notes he was learning, each designated by its vibration rate. In the last two test sessions, either 9 of the 25 or all of the 25 tones were replaced by ones near them, but different in frequency. The new tones did not produce any increase in the magnitude of error (measured in DL's) over the last training session. Wedell concluded that *O*s developed a subjective scale in which they could place unpracticed tones as well as practiced ones, and that they learned the scale rather than individual notes.

In this interesting experiment the result of *O*'s learning was to *increase* the *specificity* of the correspondence between the stimulus input and the perceptual judgment. Average error in DL's was reduced by one-half or more. One might ask how an increased specificity of stimulus-response connections can transfer to other stimuli when the very concept of transfer implies nonspecificity or generality of connections. The answer to this seeming paradox may lie in *O*'s ability to make a conceptual generalization as the differential learning proceeds, which can then mediate transfer when the particular stimuli are shifted along the dimension. Here a subjective scale with anchored and differentiated points provided a common yardstick on which new stimuli could be located. When dimensional transfer fails, the learning situation may be one which does not permit development of a useful conceptual mediator. An experiment by Gibson and Smith (**56**) may illustrate this point. The *O*s were given training in estimating

different distances. Each distance was represented by a photograph of a stake driven into the ground in a very long, level field. The *O*s soon learned to give the appropriate distance in yards for the stakes. But when size judgments of the stakes (by the method of matching with a standard) were later asked for, accuracy (that is, size constancy) was not improved by the training in distance estimation. Introspections showed that *O* was not developing a conceptual depth scale during the training at all; instead he was memorizing specific cues, such as accidental spots in the photograph, for the different estimates. Such learning could not transfer to the new judgment of size-at-a-distance.

Retention

Facts having to do with retention of perceptual learning are spotty and permit no generalization, except that forgetting over time does occur. Whether it occurs because of sheer disuse or some positive cause is not known, though Postman and Page (**138**) have demonstrated retroactive inhibition in a task involving perceptual learning. No study has secured sufficient evidence to plot a curve of the extent of forgetting with time. Degree of retention appears to be different depending on the nature of the learned performance. For instance, there is "rapid loss with disuse" of improvement in the two-point limen on the skin (**33**, **128**); but improved visual acuity was said to be retained after as long as two years (**120**). Improved peripheral acuity was said to show about 50 per cent retention after various intervals (**114**, **115**). Dallenbach reported that the effects of span-of-apprehension training in children persisted 41 weeks or longer after practice ceased (**29**, **30**). The effect of training in judgment of numerosity (**125**) was still apparent after six months, though variability rose again. An illusion which had been reduced with practice still showed a CE smaller than the original one after a year (**19**). Improvement in discrimination of absolute pitch in one study (**124**) was reported largely lost after several years of no practice. In another study of absolute pitch (**63**) 16 *O*s retested after a year showed good retention, seemingly proportional to the percentage of improvement during training. Clarification of these results awaits systematic experimentation.

Discussion

Theoretical discussions of perceptual learning have been fairly frequent in recent years, but on the whole they contribute little to the explanation of the facts presented in this paper. Helmholtz's "unconscious inference" (**70**) is the parent of most current theories. Ames and his followers (**20**, **86**, **96**) have a similar concept in the form of implicit "assumptions" which are gained from past experience and mediate perception. A given retinal pattern is said to be perceived as a chair, instead of a mass of lines or a cat's cradle or anything else, because in the past this pattern of stimulation was associated with sitting in it. The stimulation has no prognostic value for perception; instead, the perception itself is prognostic because of the assumptions which mediate it. Perceiving is the "apprehending of probable significances" (**86**, p. 290).[5]

[5] Cf. Titchener's remark "We may believe with Cattell that 'perceptions are . . . in large measure the result of experience and utility,' but they must still have a psychophysical substrate, on the one hand; and, on the other, the bare reference to utility does not explain them" (166, p. 208).

Yet the evidence for perceptual learning which has been presented in the previous pages requires the conclusion that practice results in a *closer approximation of discriminative responses to differential stimulation*. Lowered DL's for pitch, or improvement in absolute estimation of visual dimensions such as distance, both mean that perceptual judgments have changed in the direction of closer and more specific relationship to the stimulus input. How can this happen if perception is determined by the purpose of the observer and his assumptions (see **96**, pp. 88 ff.), and has only a fortuitous, noncorrelative relationship with stimulation? It seems obvious that there is a dimensional correspondence (cf. **59**, **141**), perhaps very crudely differentiated to begin with; for example, *O* reacts to a variation in tonal frequency with some change in his experience of pitch if the variation is large. With training, his perceptions become better differentiated and permit finer discriminations within the dimension. But how can "assumptions" accomplish this?

Brunswik's functional and "ecological" theory of perception (**14**, **15**) is similar to Ames's theory in assuming that any correspondence between perception and stimulation is learned. The environment provides certain ecological relationships of objects and events which the perceiver happens to experience in context or sequence with varying frequency. Perception of a given object depends on the frequency of association of "cue" and "referent" in past experience. The observer, consciously or not, makes an interpretation in terms of *probability* of referent value of the stimulation, the probability deriving from previous occurrences of this relationship. A computer analogy has been suggested for this process (**75**). This view, that perception has the function of keeping *O* in touch with a distal world by rating the incoming cues as to their validity, does not seem to account for the facts of refinement of discrimination within a dimension, at least in its present formulation.

Bruner (**13**) and Postman (**135**) have both expressed a somewhat different formulation of how learning operates in perception. "We can conceive of the perceptual process as a cycle of hypothesis—information—trial and check of hypothesis—confirmation or non-confirmation" (**135**, p. 251). Here, receptive processes are assumed to yield information in themselves; in fact, under some conditions of stimulation they provide "full information" and there is nothing to learn. The theory is designed primarily to apply to ambiguous stimulation—in itself, very difficult to define—and again does not seem to handle increased accuracy of discrimination.

Hebb's empirical theory (**69**) is aimed at the perception of form. Perception is innately organized as figure on ground—the figure has a "primitive unity"—but "identity" of a form involves a developmental process. Perception of a square or circle is slowly learned, he says, and depends originally on multiple visual fixations (pp. 34 ff.). If there are visual fixations, presumably dimensional discrimination of some kind is innate. How further differentiation within dimensions is achieved is not clear.

The theoretical problem might be formulated as follows. If we think of the stimulus variable as spread along a continuum, as it has been in

most of the cases cited here, and the response categories as bearing some relationship which can be fitted along a dimension, lowering of a variable error results in the reduction of generalization curves around points on the stimulus continuum. The solid lines in Fig. 1 (variable error) represent hypothetical generalization curves at the beginning of the experiment; the band of stimulation which may elicit a particular response is originally very wide and the S-R relationship is not very specific. The dotted lines represent hypothetical generalization curves after training; the band of stimulation which may elicit any R is narrower, that is, the S-R relationship has increased in specificity. How might this effect be achieved? A procedure of "scale training," in which stimulation at different points in the scale is responded to with a judgment by *O*, in turn followed by differential reinforcement (correction), has been effective in many of the experiments cited using the method of single stimuli. The analogy with a conditioning procedure is less direct in experiments where practice is given in relative judgment, though the results are the same. The effectiveness of progressive practice suggests that a quality hitherto not responded to in isolation is being differentiated from the total stimulus input and utilized as a cue variable.

Correction of a constant error is represented in the lower diagram of Fig. 1. It is not hard to account for, since a set to shift the judgments upward or downward may be acquired and can supplement the kind of practice already mentioned.

The above description of the theoretical problem does not apply too well to the cases classified as improved perception under conditions of impoverishment. Here there is generally a distinct pattern to be recognized and identified rather than a continuous dimension of stimulation to be differentiated. In some cases, the learning took the form of paired associates, or what might be called identification learning. An identifying response was learned to "reduced cues," that is, to fragmentary stimulation. In other cases new habits of fixation and scanning may have been important, and in still others, tricks of grouping, etc.

Increased acuity on the skin and retina must be considered in terms of the procedure used for practice. Some cases of increased visual acuity seem to resemble learning with impoverished stimulation, that is, *O* learns to respond to "reduced cues" by learning what the fragmentary stimuli represent. But in other cases, the process seems to involve learning to respond to a quality of stimulation hitherto undifferentiated. For example, reduction of the two-point limen on the skin was considered by Boring a case of *O*'s learning to use a finer criterion for identification—a better acquaintance with the difference in feel between two points and one (**8**).

The fact that there may be some transfer of improved discrimination suggests that a process of abstraction may go on with the development of differentiation. At least, *O* seems to abstract the quality common to the scale, that is, he gets a concept of the dimension of stimulation being varied. He might next, under some conditions of training, conceptualize the scale unit, the ends of the scale, and perhaps proportional intervals. An experiment by Werner on "micromelodies" (**190**) suggests such a

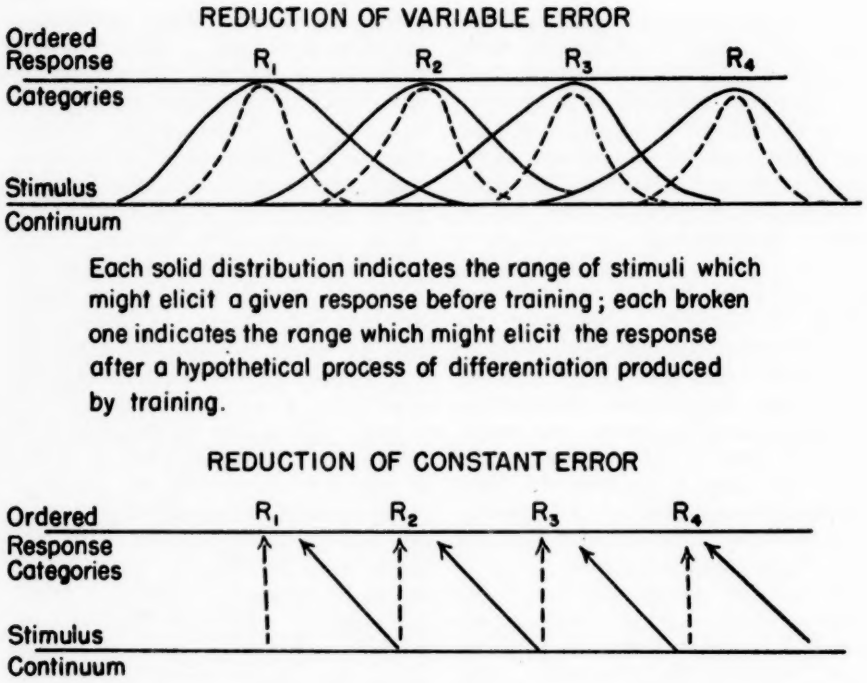
REDUCTION OF VARIABLE ERROR
Ordered Response Categories
R1
R2
R3
R4
Stimulus Continuum
Each solid distribution indicates the range of stimuli which might elicit a given response before training; each broken one indicates the range which might elicit the response after a hypothetical process of differentiation produced by training.
REDUCTION OF CONSTANT ERROR
Ordered Response Categories
R1
R2
R3
R4
Stimulus Continuum
The solid arrows indicate an S–R relationship which might exist before training; the broken arrows, a relationship which might exist after hypothetical correction of a constant error.

FIG. 1. A SCHEMATIC FORMULATION OF CHANGES WHICH MAY OCCUR IN IMPROVEMENT OF PERCEPTUAL JUDGMENTS

process. His *O*s learned a whole new microscale which had 12 distinguishable notes in a pitch range so narrow that before training the discriminations were impossible. Four *O*s learned to identify the tones with numbers when a melody was played and were able to recognize a melody transposed one tone up or down. If one tone was altered in the transposition, *O* could often tell which one it was.

That perceptual learning occurs under many conditions is clear, as also the fact that improved skill in discrimination is an important feature of such learning. Other kinds of perceptual learning also occur, such as learning to recognize and identify objects, a topic too large to have been included within the present review. An enormous array of problems invites investigation in this area and provides a program of research which may yield practical results as well as the eventual enrichment of learning theory.

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STUDIES OF DREAMING

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Reviews of the literature on dreaming have been made in the *Psychological Bulletin* by Frost (**35**, **36**) in 1915 and 1918, by Small (**101**) in 1920, and by Richards (**89**) in 1924. The reviewers included articles which were philosophical, literary, and anecdotal as well as those which were more scientific in nature. The present review is restricted to the more empirical and systematic studies on dreaming, particularly those in which some quantitative findings are reported. In general, a study is included in this review if the results presented could be checked by repetition of the investigation. This criterion for selection of articles rules out most but not all of the articles which deal with the analysis and interpretation of dreams. The American literature was rather carefully checked for data on dreaming. Undoubtedly a few reports were missed because they are occasionally buried in investigations of other topics.

A critical evaluation of many of the studies reporting quantitative data on dreaming is often difficult or impossible to make because they are so poorly reported. Some studies report results which appear to have real merit; for others the results appear questionable. The reviewer therefore adopted the policy of presenting findings whenever they were based on quantitative data even though an adequate basis for their evaluation was not included in the respective reports. Conflicting data in the various studies bring into question the validity of certain findings. In addition, future research will continue to act as a refining process in separating the verifiable data from that which is not. For the benefit of future research workers the reviewer discusses at the end of this article a few of the weaknesses he has observed in the published studies and makes a few suggestions for further investigations of the phenomenon of dreaming.

The restriction of the review to the more quantitative studies does not imply that less systematically controlled and reported observations are without value. Many insightful observations have been made in clinical practice which serve as useful hypotheses for experimental and quantitative investigations. The reviewer appreciates the clinical validity of the dynamic nature of dreaming and its relationship to the total functioning of the personality. The complex role and meaning of dreams in the life of the individual are not easily attacked at the present time by quantitative research methods. A few studies of dynamic aspects of dreaming have appeared, and more are certain to follow.

The relatively small number of major research studies on dreams is surprising since dreaming is such a universally experienced phenomenon and has through the centuries been of major interest to man. The lack of scientific interest and data on the topic has contributed to the myriads of speculations and the plethora of unscientific publications in the field. The topic of dreaming is largely shunned by current research workers and is given only the briefest of treatment or entirely omitted in most contemporary psychological textbooks.

Yet this reviewer is of the opinion that research in this area which utilizes the current knowledge of research designs and techniques could yield data which would give a better understanding of dreams. This review is therefore designed to facilitate more systematic research on dreaming by bringing together from diverse sources material bearing upon the topic and by providing a summary of the major findings which are reported therein.

Imagery in Dreams

Direct inspection of reported dream material has yielded fairly consistent data regarding the nature of imagery appearing in dreams. Studies of imagery in dreams have been made by Calkins (**13**), Weed and Hallam (**115**), Manacéine (**73**), Monroe (**80**, **81**, **82**), Andrews (**2**), Wiggam (**118**), Thomson (**106**), Bentley (**7**), Schriever (**95**), and Middleton (**77**). In studies of the dream reports obtained from normal individuals there is general agreement that visual imagery is the predominant type among all the sense modalities that appear in dreams. The second most frequent type of imagery reported is auditory. Visual and auditory imagery together are the principal elements in a large majority of all dreams. Only infrequently do tactual, motor, gustatory, and olfactory imagery occur in dream reports. For a time there was a debate on whether gustatory imagery ever appeared in dreams, but it was shown that this type of imagery did occur in case reports obtained by Titchener (**107**) and Weed and Hallam (**115**). A fairly representative set of percentages for the frequency of appearance of various types of imagery in dreams is given by Weed and Hallam (**115**). They found that out of the 381 dreams reported by six subjects, 84 per cent involved visual imagery, 67 per cent auditory, 10 per cent dermal, 6 per cent gustatory, and 6 per cent olfactory.

The reported figures for the frequency of occurrence of sense modalities in dreams are somewhat difficult to compare because of different methods of calculation. In some cases only the predominant imagery of each dream was tabulated. In other studies all types of imagery appearing in a dream were counted. Sometimes it was impossible to tell which of these two systems was used. In addition a few studies reported frequency figures for the various combinations of imagery which appeared in dreams. Even though direct comparisons cannot be made between many of the studies, the general findings, however, do appear consistent.

Considerable variation among individuals has been noted for the frequency of the various types of imagery appearing in dreams. This point was covered by Weed and Hallam (**115**), Monroe (**82**), Thomson (**106**), and later investigators. Also some variation has been noted in the frequency reports on various types of imagery given by the same individual from time to time.

Visual imagery. Most visual dreams are experienced as being in black and white. This is an interesting deviation from the daytime visual experiences of most individuals. Bentley (**7**) found that color in dreams appeared in only 20 per cent of the 54 dreams reported by five adult subjects. Among 277 college men and women, Middleton (**77**) found that 60 per cent had experienced color in dreams. Among individuals who do have color in their dreams, the frequency of such dreams is not high. Monroe (**80**) reported an

apparent increase in the frequency of colored dreams when his 14 subjects viewed geometric forms and simple objects cut from colored pieces of paper before going to sleep. Morphine has been reported by Jelliffe (**55**) to cause an increase in the number of colored dreams. Finley (**30**) claimed that a pituitary hormone given to a patient increased the frequency of colored dreams. Lovett (**68**) found that psychiatric patients reported more color dreams than did his normal controls. Ellis (**27**) cites evidence presented by Wynaendts-Franklin that among 300 adults more women than men reported having dreamed in color.

Auditory dreams. Manacéine (**73**), Calkins (**13**), and Bentley (**7**) stated that auditory dreams are primarily composed of verbal sounds and only infrequently were other auditory sounds such as bells, noises, etc. reported. Manacéine (**73**), however, stated that words, phrases, or sentences appeared in only 8.5 per cent of the dreams he studied. Most frequently the verbal sounds were words spoken by the dreamer, and only infrequently by the other participants in the dream. Some discussion has been made of exclusively auditory dreams which are reported to occur among musicians.

Gustatory. Several incidental reports appear on the effect of various gustatory stimuli upon dreams. For example, Monroe (**81**) had 22 female students place a clove on their tongues before going to sleep. In the series of dreams reported by them there appeared an increase in the frequency of gustatory elements along with an increase in olfactory elements.

Imagery of the blind. In 1838 Heermann (**47**) studied the dream imagery of the blind, and his findings were confirmed and extended by later investigators such as Jastrow (**54**), Wheeler (**117**), Deutsch (**24**), and Bolli (**12**). Jastrow's study in 1888 of 200 blind males and females still remains the basic investigation of this subject. Findings are in agreement that practically all the blind do dream, but possibly not quite so frequently as normals do. The dreams of the congenitally blind were found to be totally lacking in visual imagery, but they were highly filled with auditory and motor images. Dreams of the blind also included tactual, gustatory, organic, and other sense imagery in probably a higher frequency than found in normals. In a case cited by Wheeler (**117**) a blind individual reported the same type of synesthetic sensations in dreams as experienced in waking life.

Jastrow (**54**) and others also investigated dream imagery in individuals who became blind at various periods after birth. They reported that individuals who became blind after the first five or six years of life did maintain some visual imagery in their dreams, although they showed a progressive deterioration or fading as the individual became older. A few individuals studied by Jastrow reported visual imagery after 40 years of blindness. Those who became blind before the age of five or six did not retain visual imagery in their dreams.

Imagery of deaf and blind. Jastrow (**54**), in collaboration with Hall, reported on the dreams of a subject who was both blind and deaf from birth. Her dreams were devoid of both visual and auditory imagery. Tactual and motor imagery were the principal elements in her dreams. The people who entered her dreams "talked" with their fingers and she was observed to be "talking" with her

fingers during her sleep. The authors state, "In short, her dreams are accurately modeled upon experiences of her waking life, reproducing in detail all peculiarities of thought and action which a phenomenal education impressed upon her mind."

Imagery of the crippled. Jastrow (**54**) reported on a few cases of imagery in the dreams of the crippled. Those who lost an arm or leg sometime after birth continued to dream of the use of the lost member. For one person, such a dream continued for over 15 years. He cites Heermage as reporting a case of an individual born with only stubs for legs, and in his dreams he walked around on his knees just as he did in actual life.

Speed of Dreams

Many people have speculated on the speed and length of dreams, but as yet no conclusive evidence regarding the topic is at hand. The older writers contended that dreams were exceedingly fast, rarely lasting more than 1 or 2 seconds. Maury's (**74**) well-known dream report has been frequently cited as evidence of the speed of dreaming. Many case reports and discussions of the speed of dreams have been made since then. Ellis (**27**) reported the following early studies on the speed of dreaming: Egger (**25**), Clavière (**16**), Tobolowska (**108**), Piéron (**86**), and Foucault (**32**). In general, they reported that imagery in dreams was no more rapid than waking imagery, though the illusion of time is experienced by the dreamer in his recall and description. Woodworth (**121**) attempted to throw light on this topic by timing the speed of hypnogogic reveries and concluded they could be as fast as those reported for dream life.

Others estimate the length of dreams as much longer, ranging from 1 to 10 minutes. Hacker (**42**), from self-observation during 450 nights, concluded that dreams last up to 10 minutes. Max (**75**) found that action currents in the fingers of deaf subjects which were associated with dreaming lasted for as long as $2\frac{3}{4}$ minutes.

While hypnotic dreams have not been demonstrated to be the same as night dreams, several investigators believe there is enough similarity between them to make comparisons. Klein (**62**) reported that hypnotic dreams lasted about 1 minute. Schroetter (**96**, **97**) has hypnotized subjects to raise their hands at the beginning and at the end of induced hypnotic dreams. In this manner he found their dreams lasted from 1 minute and 20 seconds to 4 minutes and 5 seconds. Using the same method Sweetland and Quay (**105**) found the length of induced hypnotic dreams averaged about 10 to 20 seconds, the range being from 1 second to 5 minutes. Welch (**116**) demonstrated that hypnotic dreams could vary according to instructions. The length of a hypnotic dream apparently reflects the nature of instructions given by the hypnotist and the degree of suggestibility of the subject. While these experiments may not give a valid measure of the length of dreaming, they may provide some clue to the speed of imagery.

Incidence and Frequency of Dreaming

During night sleep. Several investigators have attempted to determine the frequency of dreams during various parts of the sleeping period. Calkins (**13**), DeSanctis (**22**), Bentley (**7**), and Berrien (**8**) awakened their subjects at various hours during the night and asked them if they were dreaming. The results of these

studies reveal that more dreams are recalled during the hours just before morning waking and fewest are reported during the middle portion of the night's sleep. There was no period during the night when dreams were never recalled. Berrien reported that sudden awakening of the sleeper resulted in the recall of a greater number of dreams than more gentle techniques, such as whispering to the subject. Calkins (**13**), Weed and Hallam (**115**), and DeSanctis (**22**) noted that the dreams that occurred nearer the morning were much more vivid than those occurring earlier in the night.

Over a period of time. The question as to the percentage of individuals in a group who report dreaming from night to night has also been investigated. Kleitman, Mullin, Cooperman, and Titelbaum (**63**) found that some of their subjects only rarely dreamed, others dreamed practically every night; but for the total group about 50 per cent dreamed each night. Andress (**3**) in his study of 49 students for 29 consecutive nights found that each night about 44 per cent reported dreams, the persons dreaming differing from night to night.

Age

Frequently in the literature there is mention of the belief that the frequency of dreaming varies with the age of the subjects. The data on this point are very limited, but they do seem to support a few generalizations.

Children. Since children during the first few years of life cannot make verbal reports regarding dreams, there is no way as yet to investigate the occurrence or frequency of the phenomena. Investigators who have attempted to investigate dreams of children report that their studies are complicated by the fact that the young children often do not separate daytime fantasies from dreams, and that they distort dream reports by the incorporation of daytime experiences. Some workers find that children are reluctant to report on dreams, and others report exactly the opposite. Investigators have obtained dream reports by direct questioning of children, from reports of parents, and by indirect means such as noting dream material that emerges in play.

Claims have been made for evidence of dreaming in children of even less than one year of age. Erickson (**28**) contended that he observed a pattern of behavior in an eight-month-old infant and followed its continuation for thirteen months, at which age he was then able to elicit a report of an accompanying dream. Several investigators are confident that they have obtained evidence of dream material from children between the ages of two and three.

From reports now available it is indicated that not all children dream, and that there are considerable individual differences in the frequency of dreaming. Blanchard (**11**) reported that 5 per cent of 230 children under 18 years of age seen at a child guidance clinic could not recall dreaming; another 13 per cent recalled dreaming but were unable to make any report on the nature of their dreams. Jersild, Markey, and Jersild (**56**) studied dreams of 400 children from 5 to 12 years of age and found, as did Blanchard, that slightly over 5 per cent of them did not recall dreaming. Witty and Kopel (**120**) interviewed 3,394 boys and girls from kindergarten to the eighth grade, and of these 16 per cent reported never dreaming, 63 per cent sometimes, and

21 per cent often. Blanchard (**11**) found a statistically reliable difference for more dreams recalled after age six than before, but she believed this difference resulted from the better ability of older children to recall and to report dreams. Thus the evidence at hand so far indicates that probably 5 to 15 per cent of children cannot recall dreaming.

Adults. Manacéine (**73**) and Heerwagen (**48**) contended from their investigations that young adults dream more frequently than the aged. Heerwagen found that the greatest number of dreams occur between the ages of 20 and 25. Hamilton (**45**) reported for 100 males that erotic dreams increased rapidly in number after age 11 for males until about 80 per cent reported such experiences by age 20. For the 100 females in his study age did not show such a relationship. Kinsey, Pomeroy, and Martin (**61**) also found that approximately 83 per cent of 5,300 adult males included in their report had experienced sex dreams which terminated in nocturnal orgasm. The frequency of such dreams decreased steadily in number after age 30.

Intelligence

The relationship of intelligence to frequency of dreaming has received only slight attention. In a study of children Despert (**23**) found the greatest number of dreams was reported by those with the highest intelligence quotient (IQ), the smallest by those with the lowest IQ. Not all children with high IQ's, however, reported a high frequency of dreams. Jersild *et al.* (**56**) reported contradictory findings in that the children in their study with IQ's over 120 dreamed less frequently than those with IQ's below 120. Blanchard (**11**), Kimmins (**60**), and Selling (**99**) found no relationship between intelligence and frequency of dreaming.

Very few data bear upon intelligence and frequency of dreaming among adult populations. Manacéine (**73**) collected dream reports for five years from 37 persons of varying intelligence. He concluded that the number of dreams is less in persons with low intellectual capacity than for those with greater endowment.

The whole question of intelligence and frequency of dreaming has not been adequately studied. Future investigators of this topic should give more attention to: (*a*) the problem of the validity and reliability of the intelligence measure used, (*b*) selection and description of the population studied, and (*c*) obtaining more detailed information regarding the nature of dreams such as richness and variety of content. It appears to the reviewer that the dream life of individuals should reflect the range, richness, and variety of mental activities which are associated with the higher levels of intellectual capacity.

Sex Differences

The question of sex differences in dreams has long been debated, but little systematic evidence has been collected on the topic. A survey of the studies on dreaming revealed that data were often reported without any mention of the sex of the subjects. In those few studies in which the results were analyzed according to sex, it was found that women reported a higher frequency of dreaming than men. This sex difference was cited by Chrysanthis (**15**) in a report on 1,408 students from 12 to 18 years of age, and by Middleton (**77**) for 277 college men and women. However, in another study on a similar population Middleton (**76**) found contradictory results. Jastrow

(**54**) found that blind women dream more than blind men. Heerwagen (**48**) noted in his data that unmarried women reported a greater number of dreams than did married women.

The evidence for sex differences in frequency of dreaming among children is contradictory. Foster and Anderson (**31**) found no reliable sex differences in their investigation of the number of unpleasant dreams of 519 children. On the other hand, Witty and Kopel (**120**), in their report of 3,394 children, found that girls reported a slightly greater number of dreams than did boys. Jersild *et al.* (**56**) found no sex differences in the number of pleasant or unpleasant dreams.

Sex differences in the content of dreams have also been the subject of a few investigations. Cason (**14**) and Schubert and Wagner (**98**) reported that girls dream more frequently of home, family, and friends than boys do. Boys had far more fear dreams than girls, according to Kimmins (**60**). Gahagan (**37**) reported that the only significant differences found between the dreams of 228 university men and 331 university women were: (*a*) a greater percentage of women had "physical examination dreams," and (*b*) men had a greater percentage of dreams of being nude. Middleton (**76**) also found men dream of being nude more often than women do.

Husband (**52**) studied the dream reports obtained from 25 university males and 25 university females which he obtained by personal interview. His results indicated that women's dreams were much more vivid and more emotional than men's. Women more frequently transferred their worries, love, and problems into their dreams than did the men. Women also dreamed more about their "boy friends," while men did not dream so often about their "girl friends." Hall (**44**), in an unsystematic report on over 10,000 dreams, found that women dreamed twice as often about males as females, whereas men dreamed equally about males and females. Middleton (**76**) found that women discussed their dreams with others twice as often as did the men. Both Husband (**52**) and Middleton (**76**) found sex differences in frequency and content of erotic dreams.

Content of Dreams

The content of dreams has been of primary concern to many investigators but most of their reports are descriptive in nature. Among the earlier studies on content of dreams many were concerned with specific types of content such as dreams of falling, levitation, premonition, dreams of the dead, dreams within dreams, etc. Investigators simply looked for and found examples of dream content that illustrated a particular type. In their reports on such dreams they seldom provided any systematic data as to incidence or frequency of such dreams in a given population, or presented any relationship evidence. In general most of such work is illustrative of the content that can be found in dreams, but it provides little data on which systematic studies can be based.

Practically all investigators emphasize in one way or another the congruity and continuity between daily experiences and fantasies of the subjects, and the content of the reported dreams. For example Weed and Hallam stated, "The existence of some connection between the dream world and the waking world, that is, the suggestion of the dream world by some actual experience, is traced in most of the dreams" (**44**, p. 409). Calkins

(**13**) traced 90 per cent of the content of 375 dreams of two adults to events in their waking life. She found that many more of the dreams were connected to the more unimportant recent events in their lives than to the important ones. Andrews (**4**) similarly found that nearly 90 per cent of 118 of her dreams were clearly suggested by waking experiences. Middleton (**76**) reported that in 51 per cent of the dreams of 170 college students there were some elements traceable to the previous day's experiences.

Among the investigators who have studied the content of children's dreams, a similar generalization is reported. Jersild *et al.* stated, "The content of dreams is likely to reflect any experience, wish, fear, fancy, or circumstance which occurs in the child's waking moments, and dreams are likely to reflect unpleasant events in the child's experience somewhat more frequently than pleasant ones" (**56**, p. 133). Witty and Kopel made a similar statement based on their investigation (**120**, p. 205).

The content of children's dreams has been classified by several investigators and general agreement is noted in the findings even though different systems of classification have been used. Blanchard (**11**) classified the dreams of 230 children and found in order of frequency the following content: parents, animals, fear incidences, play activities, robbers, and death. She found no relationship between content categories and the individual's IQ, mental age, or chronological age. Witty and Kopel (**120**) reported similar findings. Jersild *et al.* (**56**) classified the content of "bad" and "good" dreams. Among the former were such content categories as vicious animals, "bad" people, physical distress and injury, supernatural creatures, fires, storms, falling, being chased, etc. "Good" dreams included acquiring such things as toys, food, money, clothes, pets, and other content such as travel, play, visits with companions, etc. Despert (**23**), in a study of the dreams reported by 39 children who were clinical patients, classified the content of their dreams as follows: 75 human beings, 55 animals, and 60 inanimate objects. She found the better adjusted children in this group had more human content in their dreams than those who were less well adjusted. Kimmins (**60**) makes several comments on dream content and its relationship to age, health, etc. but gives no actual data. For example she states that children from poorer districts were found to dream more frequently about toys, food, clothes, etc. than did those from well-to-do districts.

The content of nightmare dreams of children was studied by Cason (**14**). He reported the most frequent contents were: animals, death, murder, being chased, falling, accidents, and other unpleasant experiences. He found no relationship between content and age, sex, and education. Wile (**119**) traced the content of dreams of 25 children who during their sleep yelled, cried, walked, etc., and found that the dreams connected with these disturbances were based upon actual experiences and not born of fantasy. Usually they were related to recent events which involved physical injury or punishment, or were related to psychological trauma.

Dreams of special groups. There has been a long-standing belief that special groups, such as criminals, psychopaths, prostitutes, the mentally deficient, psychotics, etc., have characteristic patterns of content in their dreams. Most of the reports on

these various groups, even when based on actual case studies, are so inadequately reported and unsystematic that little confidence can be placed in them. For example, DeSanctis (**20**, **21**) has made voluminous reports on the dreams of several special groups, but his work is certainly not objective or systematic. His reported findings need to be checked by other investigators.

The content of the dreams of criminals has received some brief attention. Selling (**99**) obtained by interview a report on the dreams of 200 juvenile delinquents and 100 convicts. He found that 80 per cent of the dreams referred to home and activities related to it. Hanks (**46**) secured a report on one dream from each of 50 convicts and found that returning home was the most frequent content topic.

Stability of content. Hall (**43**) attempted to study the stability of dream content. He classified the dreams reported in the diary of an adult male for a two-year period and then compared the dream categories for the first year against the second year. He found a high degree of consistency in the content categories and interpreted the finding as reflecting the stability of this adult's personality dynamics.

Erotic dreams. Erotic or sex dreams have been given frequent and lengthy descriptive consideration in the past, but very little quantitative data bear upon the topic. In considering reports made on sex dreams, attention must be given to the possible effects of the socially disapproved nature of the topic upon the validity of the individual's report. For example, Foster and Anderson (**31**) and Despert (**23**) stated that the children in their respective studies did not report any erotic or sex dreams. This is not surprising since they did not ask about such dreams and depended solely upon the spontaneous reports of the children upon a socially disapproved topic. The fact that certain children do experience erotic dreams was indicated by Blanchard (**11**), who found that 9 per cent of 230 children in her group reported their occurrence. A systematic study of erotic dreams of children, however, has not yet been made.

In considering erotic dreams of adults an additional problem is encountered because reporters usually fail to distinguish between data bearing upon erotic dreams and nocturnal orgasm. Dreams which accompany sexual orgasm are usually erotic in content, but may be composed entirely of other content. Also, orgasm may occur without the recall of any accompanying dream. Therefore the data for each phenomenon must not be considered as identical, as has been the assumption in some of the reports.

Hamilton (**45**) explored erotic dreams and sexual orgasm in a group of 100 males and 100 females. His data, while systematically gathered, were presented in such a manner that comparisons and interpretations based on them are often very difficult to make. But his evidence indicated that before age 20 about 75 per cent of the men had erotic dreams which accompanied orgasm, while only 3 per cent of the women reported such experiences before age 20. About 90 per cent of the men reported dreams associated with orgasm during a lifetime, while only 33 per cent of the females made a similar report. In most cases women who did experience erotic dreams and orgasm did so only after first sexual intercourse.

Middleton (**76**) stated that college men in his study reported twice as many erotic dreams as did the

women. Selling (**99**) found that among 200 juvenile delinquents 33 per cent reported erotic dreams, and among 100 adult convicts 85 per cent made a similar report. Usually erotic dreams were accompanied by nocturnal emissions in the adult group. Franklin, Schiele, Brožek, and Keys (**33**) and also Miles (**78**) noted a reduction or disappearance of sex dreams among subjects undergoing experimental semistarvation and rehabilitation.

Husband (**52**), in a very carefully conducted study, obtained data by means of personal interview on the dreams of 25 college males and 25 college females. He included several questions about erotic dreams. He found that all the males had experienced at least one erotic dream within the last year, and all but two of the females made a similar report. About 50 per cent of the subjects reported sex dreams which involved some major type of sexual activity. The sex dreams of the females were more frequently about a relationship with a close friend, while those of the males were more often about casual companions. The females reported more sex dreams when their love affairs were at a height, but for males there was no such relationship. Females had more sex dreams when their closest friend or spouse was absent, but this was not true for the males. Dreaming of "unnatural" sex relationships was reported by 14 of the 25 females and 18 of the 25 males. Sexual contacts in dreams were made with animals, children, homosexual partners, mother, sister, etc. This study shows how much socially tabooed information can be obtained by use of the personal interview when confidence in the report and the research has been established by the investigator.

External Stimulation and Dreams

From ancient times individuals have reported the influence of accidentally or intentionally introduced stimuli upon the course of dreams of the sleeper. Most individuals can recall how some light, sound, odor, or presssure, etc. has influenced the nature of his dreams. The literature of the past centuries is replete with many interesting cases of the effects of external stimulation upon the course of dreaming. Frequently cited examples include dreams of freezing when blankets fall off during the night, dreams of fire when lightning falls upon the eyelids, dreams with olfactory components when the sleeper is in a smoke-filled room, etc.

In the last century several attempts were made to determine the effect of experimentally introduced stimuli upon the course of dreams. Historically this was the first type of experimental work done on dreams. Apparently the first experiments of this nature were conducted by Maury (**74**) in 1861. During sleep he had an assistant tickle his face, pinch his neck, open perfume bottles near his nose, drop water on his forehead, etc. When awakened, he recorded the nature of his dreams. He found in many cases that his dreams showed the influence of the introduced stimulus. Several investigators since then have performed similar experiments and have obtained like results. Early experimenters who have been cited as studying the effect of external stimuli upon the course of dreams include Hildebrant (**49**), Ladd (**64**), Weed and Hallam (**115**), Clavière (**16**), Vaschide (**110**), DeSanctis (**22**), Vold (**111, 112**), Straeke (**104**), Stepanow (**103**), Poetzl (**88**), and Cubberly (**17**). A few examples from these studies will suffice to show their gen-

eral nature. Vold (**112**) conducted a series of experiments involving immobilization of extremities by means of bandages, gloves, etc., and also a series in which he moved the limbs and body position of the sleeper. He found that such techniques did have an influence upon the reported dream pattern. Stepanow (**103**) transmitted a variety of musical stimuli through a speaking tube which was connected with the sleeper's room. He made a report on the influence of the music on the nature of dreams. Cubberly (**17**) pasted small gummed labels, called "tensors," on various parts of the body after the subject had gone to sleep. He found that in approximately 95 per cent of the experiments the "tensor" stimulus had an effect upon the course of the dreams. Usually the stimulus became the core of the dream content. All these studies showed that external stimuli, if sufficiently intense, did have a definite influence upon the course of dreams during normal sleep.

A more controlled experiment was performed by Max (**75**), who studied action currents in the peripheral musculature during sleep of deaf-mutes. He reported that the onset of dreams could be detected in most instances by the appearance of large action-current responses in the arm and finger muscles. When awakened when such responses were evident, in practically all instances the subjects stated that they had been dreaming. The same subjects, when awakened at other times, usually reported they were not dreaming. He also induced dreams experimentally by external stimuli and noted accompanying action currents. The maximum action-current response connected with a dream lasted for 2¾ minutes. Normal subjects studied in the same way did not produce action currents during dreaming similar to those found for the deaf-mutes.

A study of the influence of external stimuli upon hypnotic dreams was carried out by Klein (**62**). He used ten different stimuli such as perfume, asafetida, sounds from a tuning fork, etc. to induce hypnotic dreams in a series of experiments with eight adult subjects. He found that hypnotic dreams could be induced by the various stimuli used, and that the content of the dream reflected the type of stimulus employed. The subjects were able to recall faithfully the hypnotic dreams, and they considered them as indistinguishable from night dreams. "Falling dreams" were induced by lowering or raising the body, or parts of it, when the hypnotized subject was at rest in a bed.

Personality and Dreams

Since ancient times there has existed the belief that dreams provide a basis for understanding and interpreting personality. Freud (**34**) presented the first major treatment of this topic and supported his theorizing with case materials. Today many psychiatric and psychological practitioners claim that dreams provide useful data for reaching a better understanding of personality dynamics and problems of patients. It should be pointed out that most of their observations and opinions about dreams and personality relationships have not been subjected to systematic and quantitative investigation. However, many hypotheses have emerged from their observations; some of them have been subjected to verifiable procedures and in some cases have yielded valuable data.

In survey, the studies on dreams and personality were usually found to be rather segmental investigations

and not too closely related to one another. Therefore comparison between findings from different studies cannot be easily made.

Emotions and Dreams

The emotional tone of dreams has been a subject of much discussion and a few investigations. Apparently dreams do have a very close relationship to the emotional life of the dreamer. Weed and Hallam (**115**) found that 92 per cent of the 381 dreams of six subjects were considered by these subjects as having a definite emotional tone. Other investigators have secured reports which testify to the emotional nature of most dreams, and in addition they have attempted to analyze this element of dream phenomena. For example, several investigators have inquired regarding the pleasant-unpleasant tone of dreams.

Pleasant-unpleasant tone. Apparently individuals seldom have difficulty in classifying the affective tone of dreams as either pleasant or unpleasant. Practically all investigators of this aspect of dreaming found that unpleasant dreams outnumbered pleasant ones. The following studies of this topic are representative: Calkins (**13**) found 75 per cent of 375 dreams of two adults were classified as unpleasant; Weed and Hallam (**115**) stated that 57 per cent of 381 dreams of six subjects were considered as unpleasant; and Bentley (**7**) reported that 70 per cent of 54 dreams of five adults were unpleasant. Lovett (**68**) found that unpleasant dreams were more frequent than pleasant ones in both a psychiatric group and in a normal control group.

Large individual differences have been noted in the reports on the frequency of pleasant-unpleasant dreams. Some individuals report that practically all their dreams are pleasant; others find most of theirs unpleasant. Apparently no sex differences occur in the frequency of pleasant-unpleasant dreams according to the data collected by Gahagan (**37**) on 559 university students.

Children's dreams were also found to be more frequently unpleasant than pleasant in the reports made by Jersild *et al.* (**56**), Foster and Anderson (**31**), Witty and Kopel (**120**), and Despert (**23**). The content of unpleasant dreams appeared to correspond more closely to the children's waking fears than to their actual daily experiences. Unpleasant dreams were found to have a greater tendency to recur than did other dreams. Jersild (**56**) and Witty and Kopel (**120**) did not find much change in the frequency of unpleasant dreams with increasing age. On the other hand, Foster and Anderson (**31**) found their frequency to decrease with age. These authors also reported that unpleasant dreams appear to increase during periods of illness, overexcitement, fatigue, emotional upsets, and physiological disturbances. Despert (**23**) failed to discover any relationship between health and the frequency of unpleasant dreams. Foster and Anderson (**31**) noted that children who slept alone had fewer unpleasant dreams, and in addition that the number of siblings in the family was not related to the frequency of unpleasant dreams. Jersild *et al.* (**56**) found that children with IQ's of over 120 reported more unpleasant dreams than those with lower IQ's.

Emotional perseveration. The continuation of the emotional tone of dreams into waking hours has been considered by a few investigators. Jersild *et al.* (**56**) stated that almost one-half of the children in their study were so disturbed by unpleasant

dreams that they expressed a wish that they would never dream again. Pintner and Lev (**87**) found that two-thirds of the 270 boys and 270 girls in their study had daytime worries about unpleasant night dreams. Middleton (**76**) reported that 45 per cent of the 270 adults stated that unpleasant dreams produced prolonged moods in subsequent waking periods.

Bagby (**5**) studied the effect of emotional experiences during waking hours upon dreaming. He examined a few dreams reported by an engaged couple while under "sexual stress," and concluded that the emotional stress in the waking life of these two was reflected in their dreams.

Emotional stability. An old and rather popular belief is that individuals who are emotionally unstable dream more frequently than those who have a more stable emotional life. Berrien (**8**) attempted to study this hypothesis in his 1930 investigation. He administered the Colgate Mental Hygiene Test to four subjects to obtain emotional stability-unstability ratings. He found in this small group that the emotionally unstable reported more dreams. In 1933 he conducted a similar study (**9**) in which he administered the Thurstone Personality Test and the Colgate B2 Psychoneurotic Scale to 81 university students and thereby derived a stability-unstability rating for each one. An analysis of these ratings and reported dream frequencies gave little or no support to the belief that the emotionally unstable had a greater number of dreams.

The frequency of dreaming among children classified as anxious and non-anxious was reported by Despert (**23**). From her study of 190 dreams obtained from 39 children ranging in age from two to five who were clinic patients, she concluded that all children who had frequent dreams were among the more anxious children. Not all anxious children, however, reported a high frequency of dreams.

Psychiatric Groups and Dreams

Countless contentions have been made that the dream life of various psychiatric groups has characteristic patterns. Among the many current notions regarding the dream life of particular groups can be found such beliefs as: psychopaths never dream, hysterics have more erotic dreams, prostitutes and criminals seldom dream, psychotics experience more color in dreams, etc. It is quite possible that certain psychiatric groups, because of the nature of their disorder, might have certain unique characteristics to their dream life. At present, however, there is very little verifiable data supporting the contentions. Fragmentary evidence has been published regarding the dreams of the feebleminded (**114**), delinquent (**20**), epileptic (**30**), and other populations. The findings, however, are not very convincing.

Lovett (**68**) studied the dreams of 293 psychiatric patients and 260 normals for similarities and differences. He found no significant differences in the sexual content of dreams as reported by the two groups. The members of the psychiatric group were found to have more human content in their dreams, more incidents of fear and anxiety, and greater difficulty in separating fantasy from dream material than did members of the normal group. A study of dreams of 12 schizophrenics was made by Kant (**57**). He examined 200 dreams reported by the patients and concluded that schizophrenic patients did not dream less than normals, and that no dreams appeared which were

peculiar to the schizophrenic disorder. The psychiatric group did attribute more significance to their dreams than did the normal group. The more recent reports of Trapp (**109**) and Noble (**85**) in this area are consistent with those previously cited.

Alexander and Wilson (**1**) made an attempt to relate categories of dream content with the medical diagnosis of psychosomatic patients. The dreams of 18 patients were classified into such categories as "inhibited receptive," "satisfied receptive," etc. A study was then made of the frequency of each dream category reported by patients with peptic ulcer, chronic diarrhea, constipation, etc. In general, findings were in agreement with current psychoanalytic theories of psychosomatic disorders.

Traumatic Experiences and Dreams

Many cases are cited in the literature to show the effect of traumatic experiences upon dreaming. Case reports given by Anderson (**2**), Bagby (**5**), and Gahagan (**37**) are illustrative. The traumatic experiences of war and their relationship to dream life are discussed by Grinker and Spiegel (**40**), Kardiner (**58**), and several others.

Group studies of traumatic experiences and possible relationships to dreaming are very few. Wile (**119**) selected a group of children who were experiencing violent dreams and then searched for traumatic experiences in daily life which might explain the disturbing dream life. In his study he selected 13 girls and 12 boys who cried, yelled, and fought during their sleep. He found that the violent dreams of these children appeared to be related to their actual experiences. In about 20 per cent of the cases the dreams were related to physical injury or punishment, while 80 per cent were centered around traumatic psychological experiences.

Visual Perception and Dreams

A few attempts have been made to study the effect of the presentation of types of visual stimuli during waking periods upon subsequent dream material. Malamud and Linder (**72**) cite such an experiment conducted by Poetzl (**88**) in 1918. He had pictures presented tachistoscopically for 0.1 second to normal subjects and then asked them to report what they saw. Then they were told to make a record of any dream they had about the pictures. The subjects brought out more details of the pictures in the dream reports than were mentioned after viewing the pictures. He explained the difference in immediate recall and in dreams as a function of personality dynamics.

Vold (**111**) gazed at small plastic objects or figures before going to sleep and then studied his dreams in order to detect what influence they had upon their course and content He reported that very seldom did the form, size, or color of the object remain unchanged, but that the object often appeared in some alternative or distorted manner.

Malamud and Linder (**72**) compared the recall of visual materials immediately after presentation with the dream reports of the same stimulus. They presented a series of pictures, one at a time, for 30 seconds to 27 male and 20 female psychiatric patients. After each presentation of a picture a five-minute period of conversation intervened. The subject was then asked for a complete description of the picture. The patient was then told to dream of the picture and, if he did, to write a report of it. The investigators found that some content which did not appear in the

immediate recall after presentation did appear in subsequent dream reports. They contended that the details recalled or omitted were related to the individual's early personality development and the current psychiatric disorder.

Sarason (**92**) compared the Thematic Apperception Test stories obtained from 25 mentally defective females with dreams reported by each individual. He reported that a similarity was found between the two sets of data. He pointed out that not in every case were all the major themas in the thematic materials found also in the dreams, but that in no case were data from the two sources at complete variance. Griffiths (**39**) found a striking similarity between usual dream images of subjects and responses obtained from inkblot reactions.

Ego Involvement and Dreams

Sweetland and Quay (**105**) investigated the influence of "conceptual" stimuli upon the nature of hypnotic dreams. They prepared a list of 40 ego-involving and 10 neutral statements. These were presented, one at a time, to hypnotized subjects with the instructions "to dream" to each one. For 16 college subjects they found that reaction time and duration of dreams had no relationship to the ego-involving stimuli. The data did show, however, that each subject had his own characteristic speed and duration of dreams. Recall of dream material was found to be a function of recency, and to be independent of the nature of the stimulus.

Symbolization. Freudian theory (**34**) gives considerable attention to the symbolization which occurs in dreams and the interpretation of such materials. A few investigations of symbolization in hypnotic dreams have been conducted, and the results may throw some light on symbolization which occurs in night dreams. Schroetter (**96**), as early as 1911, studied the effect of direct and indirect representation of suggestions upon the nature of hypnotic dreams. He prepared a series of statements which suggested wish fulfillment, clang associations, abnormally small dimensions of dream figures, and various types of sexual activity. These statements were read one at a time to each hypnotized subject. After each presentation the subject was instructed "to dream" about it. A report is given of the dream material obtained from such experiments. The investigator points out the symbolic representations in the dream productions. Negative results were probably encountered but they are not reported. Roffenstein (**90**) conducted a very similar investigation. He instructed a 28-year-old, uneducated female to dream symbolically while under hypnosis of fellatio, homosexual intercourse, rape, and intercourse with her father. The dream material obtained is reported. The investigator points out symbolization in the dream content and discusses its symbolic significance.

Symbolization or alteration of stimulus material in hypnotic dreams and its relationship to emotional adjustment of the dreamer were reported by Sweetland and Quay (**105**). They presented 16 college students with a series of ego-involving statements as topics for hypnotic dreams. The resulting dreams were classified into symbolic and nonsymbolic categories. The best adjusted subjects, as indicated by two psychological tests, had the greatest number of symbolic dreams. Only a slight relationship appeared between intelligence and

frequency of symbolization.

Nachmansohn (**84**) attempted to study dream symbolization, dream censorship, and dreams as a protector of sleep by use of the posthypnotic suggestion "to dream" during regular night sleep. He prepared a series of statements which served as stimuli for induced hypnotic dreams. After reading a statement to the hypnotized subject he instructed him "to dream" in a symbolic manner to the stimulus. Censorship in dreams was similarly investigated. Also he attempted to show how dreams act as a protector of sleep by a demonstration using the posthypnotic suggestion "to dream" during night sleep. He reports on a subject who had been awakening each night from a recurrent headache. He gave the subject a posthypnotic suggestion "to dream" at night of the headache. He claimed that as long as the posthypnotic suggestion was used, the patient did not awaken at night.

Farber and Fisher (**29**) studied symbolization occurring in hypnotically induced dreams by using both sexual and nonsexual suggestions for dream stimuli. They pointed out the wide range that occurred in the nature of the symbolization. They argued against a too narrow interpretation of the process.

Physiology and Dreams

Physiological studies bearing on dreaming can be separated for discussion purposes into two groups. In the first group are placed those investigations which report data bearing upon some physiological activity that occurs while dreaming is in process. Considered under this grouping are such phenomena as brain potentials, action currents, galvanic skin responses, blood pressure, etc. The second group includes studies which report that certain physiological conditions have an influence upon the nature and content of dreams.

Physiological Activity during Dreaming

One major problem encountered in physiological studies is establishing the fact that the sleeper is or is not dreaming. Investigators have had to rely upon awakening subjects at times when selected physiological data appeared and asking the subject if he was dreaming. Using this method a few studies have been made which provide some data bearing upon physiological activity during dreaming.

Brain potentials. Loomis, Harvey, and Hobart (**65, 66, 67**) and Davis, Davis, Loomis, Harvey and Hobart (**19**) obtained records of brain potentials during sleep. These were analyzed according to reports of the subject as to whether or not dreaming had occurred prior to awakening. Loomis *et al.* reported: "We are now inclined to believe dreams are not likely to occur with any unusual pattern of electrical potentials but with a state of sleep" (**66**, p. 142). Davis *et al.* make a similar report: "We have no evidence relating dreams to specific changes in the record" (**19**, p. 33). They reported that dreaming does occur in both the B state of electroencephalogram (EEG) recordings (low voltage, and alpha rhythm is lost) and C state (spindles: short groups of 14-per-second waves appear and also random "delta" waves 0.2 second or more in length). Concerning the C and D states they have no report.

Blake and Gerard (**10**) found the presence of delta waves to be related either to dreamless sleep or to the inability to recall having dreamed.

For one subject it was found in nine-tenths of the trials that if alpha waves were missing for nine seconds and no delta waves were present, the subject could remember a dream when awakened. Sirna (**100**) made EEG recordings of hypnotically induced dreams and found that the physiological changes during hypnotic dreams show no positive correlations with physiological changes to be seen in the normal sleeping dream.

Muscular action currents. As indicated previously, Max (**75**) found an interesting relationship between action-current records taken from the peripheral musculature of sleeping deaf subjects and the reports, gathered on awaking, of the presence or absence of dreams. The onset of dreams could be detected in most instances by the appearance of large action-current responses in the arm and finger muscles. Such currents were usually unaccompanied by overt muscular movements. Also, the author investigated hypnotic dreams which were induced by sensory stimulation. The same type of accompanying action currents were noted as appeared during night dreams. The investigation was repeated using normal subjects, but negative results were obtained.

Galvanic skin responses. No studies of galvanic skin responses during night dreaming were found. Ikin, Pear, and Thouless (**53**) investigated galvanic responses made by waking subjects when asked to free-associate to materials drawn from their own dreams. The data showed that memories of emotional dream materials did cause noticeable GSR's. In one case recall of a dream which occurred over 15 years before produced a definite response.

Circulatory activity. MacWilliam (**71**) studied blood pressure and heart action in sleep and dreaming. He found that the systolic blood pressure could rise during a dream from 130 to over 200 mm. He further stated that while the most striking cardiovascular effects are found in dreams with strong emotional content, vivid dreams of active movement (such as cycling) may be associated with a pronounced rise in blood pressure.

Respiratory activity. Rowland (**91**) obtained pneumographic records while subjects under hypnosis were instructed to dream of a series of emotional rated words. Each word had previously been rated by judges as to the degree of emotional value it usually carried. The pneumographic records showed that the increase in irregularity of breathing was roughly proportional to the scaled value of each word.

Gastric contractions. The relationship between hunger contractions of the stomach and reports on dreaming have shown a positive correlation in the reports made by Wada (**113**) and Scantlebury, Frick, and Patterson (**93, 94**). The stomach movements in these studies were studied by use of a gastric balloon. At certain times during hunger contractions and quiescence, subjects were awakened and asked if they were dreaming. The evidence showed that dreams occur only in the period of active hunger contractions. No dreams were found during the quiescent period. Scantlebury *et al.* (**94**) also investigated the effects of hypnotically induced dreams and found that when food was a part of the dream content it had an inhibitory influence upon stomach movements. McGlade (**70**) provided evidence to show that dreaming occurred at the end of a series of twitches in peripheral members of the body which accompanied the evacuation of the stomach.

Physiological Conditions Influencing Dreaming

Several writers contend that certain physiological tensions, disequilibria, or dysfunctioning influence the nature and content of dreams. Claims are made that such conditions as hunger, thirst, fatigue, sexual deprivation, bladder and bowel distention, respiratory and circulatory irregularities, organic diseases, etc. produce alterations or special characteristics in dream patterns. Other factors falling into this topic are the effects of drugs, hormones, narcotics, etc., upon dreaming. The amount of evidence supporting many of these beliefs is meager or totally lacking.

Hunger and dreams. The belief that hunger or periods of starvation increase the frequency and content of dreams pertaining to food and eating is frequently met. There is some support for the belief from a few studies. Several anthropologists have reported that members of tribes who are exposed frequently to hunger have at such times an unusually large number of dreams about food gathering, preparation, and eating. An example of such a report is made by Holmberg (**51**) in his study of the Siriono Indians.

Benedict, Miles, Roth, and Smith (**6**) and Sorokin (**102**) investigated the dreams of hungry persons and reported that they are primarily centered around food and eating. However, in both of these studies the methods of investigating frequency of dreaming were so questionable that little confidence can be placed in their reports.

Contradictory reports on the influence of hunger and starvation upon dream patterns are found in the University of Minnesota starvation experiment carried out by Keys, Brožek, Henschel, and Mickelson (**59**). In this more carefully conducted study a report was obtained periodically from 36 men who were undergoing a starvation experiment. This study extended over a period of 20 weeks. The investigators found that dreams of food were rare, and no marked tendency was noted for them to increase during the period of starvation or during the period of rehabilitation which followed.

As reported earlier, Miles (**78**) and Franklin *et al.* (**33**) reported that sex dreams were greatly attenuated or entirely eliminated when subjects were undergoing reduction in normal diet or under semistarvation experiments.

Anoxia. Lovett (**68**) cites a study by Monge (**79**), who reported an increase of disturbing dreams when dwellers of high altitudes suffer from chronic mountain sickness. McFarland (**69**) reported on dreaming among members of an expedition going from sea level to high altitudes in the Andes. The generalizations made from their reports were that while ascending, dreams increased in frequency, were unusually fantastic and illusory, and a shift in content was noted from sex and home to worries about health. But at elevations of over 17,000 feet dreams practically never occurred, even with nocturnal emissions. The conclusions drawn were that the general physiological state appeared to be equally as important as inner conflicts or motives in determining the nature of dreams. Lovett (**68**) found no significant relationships when he correlated the arterial oxygen saturation expressed in "reduction-time" seconds and the frequency of nightmares, color in dreams, and recall of dreams.

Hormones. Finley (**30**) reported a case in which endocrine therapy had a marked influence on dreaming. When the subject was being given one grain of the extract of whole pituitary gland daily, there was a large increase in the frequency of her dreams. In addition the dreams were much more vivid, pleasant, full of action, and included many colored ones. Increase in ability to recall dreams under these conditions was also noted. When the patient was changed to suprarenal gland therapy fewer dreams were reported, and most of them were unpleasant. There was no color in the dreams and recall was difficult. Under this hormone she would awaken at night from startling dreams and would exhibit considerable emotional disturbance. At the termination of therapy she returned to her normal dream pattern. The patient reported an increase in the number of pleasant dreams around her menstrual period. Daniels (**19**) studied the occurrence of erotic dreams of three females during the menstrual month and found that, while such dreams appeared throughout the month, they showed a concentration around the supposed time of ovulation.

Sensory disturbance. Hoff and Pötzl (**50**) reported on the dreams of two subjects who had central and labyrinthine disturbances. An unusually large number of their dreams were of falling and flying. Grünbaum (**41**) and Eisinger and Schilder (**26**) cited cases in which a tumor or organic involvement of the auditory nerve produced a high incidence of auditory dreams.

Enuresis. Investigators (**83**) who have studied the dreams of enuretic children have reported that these children often experience vivid and realistic "toilet dreams" just before or during the act of urination. These reporters have failed, however, to show any difference in such dreams of enuretics and normal children. Many children and adults also experience "toilet dreams," but do awaken before urination.

Narcotics and drugs. Many writers have discussed the production of hallucinatory imagery by use of drugs, anesthetics, narcotics, and other pharmaceutical agents. Most of this material, however, makes no mention of their influence upon night dreams. One study by Jelliffe (**55**) reported the increase of color in night dreams when the subjects were under the influence of morphine. Another author mentioned that drug addicts, when undergoing withdrawal treatment, dream frequently of seeking narcotics. The question of the influence of drugs and narcotics upon night dreaming, however, has not been systematically investigated.

Evaluation of Research

There are only a few research articles on dreaming which have been so designed and reported that they can be repeated and published findings thereby checked. Future studies on this topic need to give greater consideration to the simple research criterion of repeatability if systematic knowledge of dreaming is to be extended. In the past most of the studies on dreaming have been limited to case reports and speculations. While these have some value in themselves, they should lead eventually to more quantitative types of investigations. This goal, however, has not been served by most of the past studies.

A very noticeable weakness in many of the past investigations is the quality of scientific reporting. Often only findings or generalizations are

reported even though they are based on a respectable body of data. Unless essential basic data are presented, future research workers will be unable to verify and extend past results. Presenting basic data also makes it possible for future researchers to separate actual findings from interpretations. Too frequently a reviewer of the studies on dreaming simply cannot tell whether statements made by an investigator are based on fact or are a product of his speculations.

Investigations of dreaming are particularly weak when it comes to describing the nature of the population on which they are based. Many investigators mention only the number of subjects involved in a study and seldom add any information about sex, age, level of intelligence, health, economic status, education, etc. Yet the literature is full of speculations as to the influence of such variables upon the nature and content of dreams. Many of the studies have been based upon a very limited or select group of subjects. Findings reported from such studies should be considered as tentative or exploratory. Too often they are treated as definitive or used as the basis for generalizations which are not justified.

Another weakness is noted in the failure to employ control groups when comparative statements are made about one or more selected groups. In the literature claims are frequently made that particular groups such as the feebleminded, prostitutes, criminals, psychotics, the aged, etc. have characteristic dreams. Seldom do such studies provide any comparative data from a control group. Therefore claimed characteristics of groups are not adequately established.

More precise definitions are needed in selecting and classifying various aspects or characteristics of dreams. Some of the contradictory findings appear to be artifacts of the classificatory treatment of data rather than actual characteristics of dreams. For example, contradictory evidence was found regarding "recurrent dreams." Yet in the studies no definition of the category was given. Other concepts needing definitions so that results can be checked would include such terms as symbolization, censorship, projection, categories for classifying dreams, etc.

A survey of past studies on dreaming probably gives one of the best examples in the field of psychology for the need of statistical controls and treatment of data. Many of the past workers failed to use the statistical procedures and checks available to them. Present day investigators of dreaming have many more and better statistical tools to apply to research on this topic. Statistical tests of validity, reliability, significance of differences, population sampling, etc. must be employed when applicable or the research involved is subject to sharp criticism.

A large portion of the data on dreaming is based on individual interviewing. Such a procedure has inherent and potential weaknesses, but like any method there are degrees of control which can be exercised over many of these factors. The researcher should be aware of possible distortion or bias that might appear in data collected by personal interview. At the same time it should be recognized that the interview method has contributed much valuable data and insight into the phenomenon of dreaming.

The subject of dreaming is considered relevant to the dynamics of

behavior by many practicing psychiatrists and psychologists. Even academic psychologists who are interested in thought and imagery processes find that dreaming is a phenomenon of importance to them. The psychologist with his research orientation and research abilities could undoubtedly help advance the knowledge of dreaming from the present speculative and descriptive stage and give it a more experimental and quantitative basis.

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ASSESSING SIMILARITY BETWEEN PROFILES[1]

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A great many current investigations, particularly in clinical and social psychology, deal with similarity between profiles of test scores. Such studies vary widely with regard to the problems posed and the specific variables used, but they have in common an attempt to deal with several scores or traits simultaneously. Some investigators attempt to identify "types" of people who have similar configurations of scores. Much of so-called inverse factor analysis has this aim. Other studies attempt to differentiate clinical or occupational groups by means of patterns of test scores (e.g., **1, 28**). In another type of problem, two or more profiles for the same person are compared. The person is assessed more than once on the same set of variables, and the consistency of the profiles is measured. This is one method used to study the validity of clinical procedures (**5, 24**). Profile comparison also permits exploration of new variables such as self-consistency over time (**31**) and assumed similarity in perception of others (**17**).

At present many techniques are available to the investigator who is concerned with assessing the degree of profile similarity. The method most widely known among psychologists is that of correlating one profile with another, generally termed a *Q* correlation. Burt (**3**) and Stephenson (**32**) have been chiefly responsible for developing this approach.[2] Special indices have also been proposed, such as the coefficients of pattern similarity of Cattell and those of du Mas. A distance measure has been described recently by Osgood and Suci (**26**) and by the present writers (**11**).[3]

A very valuable summary of statistical literature bearing on the use of profiles or patterns to classify individuals into relatively homogeneous groups has been prepared by Hodges (**22**). Other recent reviews which deal in part with this problem are Gaier and Lee's (**20**) and Tyler's (**35**).

The various available methods of measuring profile similarity yield somewhat different results. Proper choice of a measure for a specific investigation requires knowledge of the assumptions, limitations, and information utilized in the several methods of measuring profile similarity. *It appears that the methods most often used have serious limitations. Much superior methods can be proposed.*

We intend in this paper to examine in a general way the problem of comparing sets of scores and to clarify the mathematical logic involved

[1] The study was supported under Contract N6ori-07135 between the Office of Naval Research and the University of Illinois. The first version of this paper was presented to the Midwestern Psychological Association on April 27, 1952, and a more detailed technical report on the material (**11**) was issued in April, 1952.

[2] Stephenson's current work on *Q* technique (**33**) departs from the correlational methods reviewed here. We shall not discuss here the logic of his basically new approach using analysis of variance.

[3] The work of Osgood and Suci (**26**), and our own work, was in large measure independent. While working on our separate problems, however, we exchanged ideas occasionally, and found our interests converging on the *D* measure. We appreciate their cooperation and that of others who have discussed our problem with us.

therein. This permits us to consider the various formulas which have been advanced in the past, and to draw attention to those approaches which seem to have greatest merit.

This paper is primarily concerned with *descriptive* indices applicable to the investigation of questions such as the following:

1. How similar are Persons 1 and 2?
2. How similar is Person 1 to Group *Y*?
3. How homogeneous are the members of Group *Y*?
4. How similar is Group *Y* to Group *Z*?
5. How much more homogeneous is Group *Y* than Group *Z*? Than the combined sample?

Comparable questions may be asked in studies concerned with two or more profiles for the same person.

While it is necessary to describe the degree of similarity between score sets in many of the investigations now being pursued, it is often equally or more important to test hypotheses such as "Group *Y* and Group *Z* can be regarded as samples from the same population" or "Individual 1 is more likely to be a member of Group *Y* than of Group *Z*." Such problems of *inferential* statistics relevant to multivariate analysis have been thoroughly studied by Fisher, Hotelling, and the Calcutta school, and several significance tests are available for normally distributed variables (**29**). We shall not discuss the inferential problems, being concerned solely with descriptive formulas for reporting *degree* of similarity.

General Methodological Difficulties

While the procedures permitted to the investigator of profile similarity are varied, they involve numerous pitfalls. We shall discuss some of these difficulties as a preliminary to formal analysis of profile comparison methods.

Similarity as a general quality. Thinking of persons as "similar" or "dissimilar" is a common oversimplification. This attractive notion, however, does violence to a fundamental principle. If behavior is described in terms of independent dimensions, then persons who are similar in one dimension may be no more similar in some second dimension than persons who are dissimilar in the first dimension. *In other words, similarity is not a general quality. It is possible to discuss similarity only with respect to specified dimensions (or complex characteristics).* This means that the investigator who finds that people are similar in some set of scores cannot assume that they are similar in general. He could begin to discuss general similarity only if his original measurement covered all or a large proportion of the significant dimensions of personality. Thus any problem inquiring whether similar people perform differently from dissimilar people must be stated in terms of the question "Similar in what?" It is most unlikely that similarity in every quality has the same effect.

Reduction of the configuration by similarity indices. Many investigators are attracted to profile similarity studies because they believe that in this way they can take into account the entire configuration of scores. However, when we try to treat a set of scores by any of the mathematical methods now being used, we no longer study the entire configuration. Instead, by reducing the configuration or the relationship between two configurations to a single index, we discard much of the information in the score set.

We may illustrate this by referring to Gage's study of insight (**19**). He asked a teacher to predict the responses of a pupil. He scored the

predictions using the responses actually given by the pupil as a key, thus estimating the accuracy of the teacher's perception. It is obvious, however, that a more refined question could be asked regarding the teacher's ability to perceive separate aspects of the pupil. In using a total index, Gage was forced to combine these many separate aspects of insight into an over-all score. It is important that the investigator recognize the limitations of so-called global approaches even though they may be the best for him to use in initial exploration of a particular area.

Absolute interpretation of index. Another type of difficulty which frequently complicates interpretation of profile similarity studies is the failure to recognize that the magnitude of the similarity index has no meaning in itself. In conventional psychometrics, we would not give serious attention to the absolute value of a test score. When we compare a person to a key, the number of items on which he and the key agree is a form of correlation. We are aware that we should not interpret this raw score which reflects the difficulty of the items. Instead, we give our attention to the relative standing of the individual in some reference group. Correlations between persons, and other similarity indices, entail precisely the same problem. Too often, accustomed to interpreting correlations as absolute numbers, investigators interpret similarity indices without recognizing that they also depend upon the difficulty or popularity of the items or tests.

One often cited study by Fosberg shows the fallacy of this type of interpretation (**18**). Fosberg hoped to demonstrate that the Rorschach test is proof against faking. He therefore asked individuals to take the test in the normal manner, and then to take it attempting to make the best possible impression. He correlated the two psychograms for a given individual and interpreted the resulting high correlations as showing that the Rorschach was proof against faking. Now it is true that the psychogram under "fake good" conditions could be predicted from that under normal conditions. But the "fake good" psychogram could have been predicted quite well from the psychogram of some other person chosen at random. Between any two Rorschach records taken at random, there will tend to be a high correlation just because certain scores (e.g., *D*, *F*) will usually be large, and other scores (e.g., *m*, *cF*) will usually be small.

It is evident that any estimate of the similarity of particular profiles must be evaluated relative to the similarity of people in general on the measures in question. A high index of similarity between two persons might indicate that they are unusually alike, or might indicate that they possess in common only the characteristics most humans have. For example, Gage (**19**) considered insight to be indicated by a marked similarity between prediction and actual response. He found that a large part of the correlation between predicted response and actual response was accounted for by the teacher's ability to predict the responses of pupils in general. When the teacher was asked to predict the *average* response of pupils, the correlation with the actual responses of an individual pupil was frequently as high as when the teacher attempted to predict that particular pupil's response.

Noncomparability of scale units. Combining many traits into any sort of composite index, whether it be a *D* measure, a *Q* correlation, a discriminant function, or any of the

other methods presently used, involves assumptions regarding the scale of measurement which usually cannot be defended (**7**, **8**). If, for example, one score measures intelligence and a second one reflects anxiety level, any single index based on this profile involves an assumption that one unit of intelligence is equivalent to some number of units on the anxiety scale. Such an assumption is perhaps necessary if it permits investigations which would otherwise be impossible. It may also be possible to justify the units assigned to the respective scales by a mathematical treatment which selects the weights to maximize some prediction. This is an empirical solution, however, and does not contribute directly to development of theory.

A Generalized Concept of Pattern Similarity

We now introduce a model for the concept of similarity between persons which provides a basis for systematic discussion of the assumptions underlying most of the common measures of profile similarity.

A profile or pattern pertaining to a person consists of a set of scores. We shall use the following notation:

j = any of the variates a, b, c, $\cdots$ which are k in number;
i = any one of the persons 1, 2, $\cdots$ N;
x_{ji} = the score of person i on variate j.

Considering only two persons, we have the set of $x_{j1}(x_{a1}, x_{b1}, \cdots x_{k1})$ for person 1, and the set of x_{j2} for person 2. Without placing any restriction upon our data, we may regard the x_{j1} as the coordinates of a point P_1 in k-dimensional space. The x_{j2} define a point P_2. The more similar the measures of two individuals the closer will their points lie in the k-dimensional space, and, conversely, the further apart the points the more dissimilar are the corresponding measurements. Accordingly we define the *dissimilarity* of two individuals as the linear distance between their respective points.

If we represent the variables by orthogonal axes, the distance D between any two points may be easily obtained by use of the generalized Pythagorean rule,

$$D_{12}^2 = \sum_{j=1}^{k} (x_{j1} - x_{j2})^2. \qquad [1]$$

D^2 can be used directly as a measure of similarity. In most cases, however, it is preferable to obtain D, since the larger differences between persons are much exaggerated in squaring. D is less skewed than D^2 but is not normally distributed.

Formula [1] is a general expression for the dissimilarity between two profiles. It may be applied to practically any type of score set; viz., responses to a series of items, raw scores on a set of tests, profiles of deviation scores, ratings of a group of stimuli on a subjective scale, or responses in a Stephenson forced-sort procedure. While formula [1] results in a measure of dissimilarity no matter what types of scores are used, the interpretation of the results depends on the nature of the scores.

One basic decision made by the investigator is whether to work with the original score set or to convert it by centering about the person's mean or by standardizing within the person. He may make these conversions before using formula [1]. Many of the current formulas automatically introduce some such treatment of scores. Converted scores in general alter the domain within which similarity is measured and consequently alter the results.

Elevation and scatter within profiles. A set of k scores, whether expressed in raw or standard measure, has k degrees of freedom and may be considered as a configuration in k space. When the profile is expressed as a set of deviations about the person's mean or when the profile is standardized within the person, the number of degrees of freedom is reduced. This has important consequences. In order to discuss them we introduce the terms *elevation*, *scatter*, and *shape*. *Elevation* is the mean of all scores for a given person. *Scatter* is the square root of the sum of squares of the individual's deviation scores about his own mean; that is, it is the standard deviation within the profile, multiplied by $\sqrt{k}$. *Shape* is the residual information in the score set after equating profiles for both elevation and scatter. We can clarify these terms by introducing numerical illustrations. Suppose that we have five traits a, b, c, d, e, and persons A, B, and C.

	a	b	c	d	e
A	2	−2	0	3	2
B	0	−4	−2	1	0
C	3	−1	3	−1	−4

According to formula [1], D_{AB}^2 is 20. $D_{AC}^2 = D_{BC}^2 = 63$.

Elevation is determined by averaging the scores for each individual. For the example above, the elevations are as follows: A, 1; B, −1; C, 0. Removing elevation, the individual profiles become:

	a	b	c	d	e
A	1	−3	−1	2	1
B	1	−3	−1	2	1
C	3	−1	3	−1	−4

Now the distance between A and B is 0. Those persons who are different when their total profiles are taken into account are indistinguishable on the basis of their profiles of deviation scores. D_{AC}^2 and D_{BC}^2 now equal 58.

The operation of eliminating differences in elevation from the profiles is referred to by Thomson (**34**) and others as centering about persons. Geometrically, it is equivalent to projecting all persons into a $k-1$ space orthogonal to the line defined by the equations $a = b = c \cdots$. Comparison of deviation scores is involved in testing certain hypotheses regarding scatter in mental tests (**2**). Burt eliminates elevation when he obtains a matrix of covariances between profiles for use in factoring persons into types (**3**). If we use D' as a symbol for distance between profiles after projection into $k-1$ space, we have the following equation:

$$D'^2_{12} = D^2_{12} - k\Delta^2 El_{12} \qquad [2]$$

Here ΔEl represents the difference in elevation between the two persons. It is evident that the difference between persons has two components, one due to elevation, and one due to the remaining information in the profile. Treatment of deviation scores discards information about differences in elevation.

When differences in scatter between profiles are eliminated, the measure of similarity is reduced to a consideration of shape alone. This is accomplished by dividing each deviation score by the individual's scatter, thus standardizing the profile. Geometrically, this operation amounts to projecting every score set in $k-1$ space onto a $k-2$ hypersphere. The center of the hypersphere is at the point representing in $k-1$ space a completely flat profile.

If for each of the three persons in the above example we divide his deviation profile by his scatter, we obtain the following new profiles:

	a	*b*	*c*	*d*	*e*
A	1/4	−3/4	−1/4	1/2	1/4
B	1/4	−3/4	−1/4	1/2	1/4
C	1/2	−1/6	1/2	−1/6	−2/3

Now $D_{AC}^2 = D_{BC}^2 = 2.25$. $D_{AB}^2 = 0$ as before.

Letting D'' be our symbol for the D measure obtained from two standardized score sets,

$$D''^2 = \frac{D'^2 - \Delta^2 S}{S_1 S_2} = \frac{D^2 - k\Delta^2 El - \Delta^2 S}{S_1 S_2}. \quad [3]$$

Here S is e scatter of an individual and ΔS is the difference in scatter. It is clear from this equation that by standardizing the profile we eliminate from consideration one further type of difference between the persons.

Elevation and scatter have commonly been eliminated in past studies of similarity between persons. It is easily shown that

$$D''^2 = 2(1 - Q) \quad [4]$$

where Q is the product-moment correlation between scores. It will be recalled that in product-moment correlation, one subtracts the product of means from the cross-product terms, and divides by the standard deviations (which are proportional to the measures of scatter). In other words, all correlations between profiles are essentially measures of distance in $k-2$ space.

Equations [3] and [4] make clear that D in k space will, in general, not give the same result as Q for a given pair of score sets, nor can D be inferred from factor loadings derived from Q correlations. Osgood and Suci (**26**) demonstrated close correspondence between the two sorts of measures, but only for an unusual set of data where ΔEl and ΔS are small. Warrington (**36**) determined the extent to which information is discarded in various treatments by building hypothetical data from a mathematical model. For his analysis, he employed five factors, represented with varied loadings in 60 items. Each of his hypothetical persons was assigned scores, distances between persons were determined, and these were correlated with distance measures based on the factor scores. For perfectly reliable items the similarity measures correlated .92 with the criterion. This "validity" dropped to .85 when elevation was removed from the similarity index but not from the criterion. For items of moderate reliability, the validity dropped from .81 to .55 when elevation was removed.

Relation of Other Formulas to the *D* Measure

Table 1 lists the formulas most frequently used in psychological investigations of profile similarity, together with some of their more prominent characteristics.

Treatments in k *space.* The D measure presented in formula [1] considers all k dimensions in the original data. This measure has recently been discussed by Osgood and Suci (**26**), but a quite similar formula appeared in the literature much earlier, as Pearson's "coefficient of racial likeness" (CRL) (**27**), which was developed to measure the similarity between two groups or the similarity of an individual to a group. In its original form, CRL was essentially the same as D^2 save that all variates were expressed in standard measure and a multiplier involving the number of cases per group was included.

The Pearson index proved unsatisfactory in the anthropological research for which it was developed. Some of the criticisms arise out of its insensitivity to differences of number of cases from group to group. These

criticisms are irrelevant to our present purpose. Morand (see Rao, **30**) notes that in some anthropological research the index has given unreasonable results for groups which were regarded as quite dissimilar, intuitively or theoretically. From the context, we judge that this difficulty is a consequence of the high weight CRL assigns to general factors among the variates. This problem may arise in measures of similarity whenever variates are intercorrelated. We shall discuss the problem of correlation in more detail later.

Cattell (**6**) has proposed an index r_p which is like D in many respects. He introduces a transformation, however, which makes the obtained index range from 1 to -1. In our notation,

$$r_p = \frac{K\Sigma\sigma_j^2 - kD^2}{K\Sigma\sigma_j^2 + kD^2} \qquad [5]$$

where K represents twice the median χ^2 corresponding to the given number of variates. D or r_p would give the same results so far as the *ordering* of dissimilarity is concerned.

Cattell arrived at this index because he believes that similarity should be measured by an index which is comparable to a correlation. This assumption seems to us neither necessary nor desirable. If persons fall into a multivariate normal distribution there should be very many similar pairs, and relatively few pairs who are far from each other. Furthermore, if we are dealing with variates having an unlimited range then no matter how far apart person 1 is from person 2, there is no theoretical reason why there cannot be a person 3 such that $P_1P_3 > P_1P_2$. Therefore we see no reason why the measure of separation should have a limit. "Complete dissimilarity of persons" is an undefinable concept.

Webster (**37**) proposed that intraclass r might have advantages for measuring similarity in k space.

$$r_{in} = 1 - \frac{D_{12}^2}{S_1^2 + S_2^2 + \frac{1}{2}k\Delta^2 El} \cdot \qquad [6]$$

The denominator in [6] is the sum of squares of scores of both persons about the grand mean of their scores. The larger this denominator the closer will r_{in} approach $+1$ for pairs having the same D. To illustrate, con-

TABLE 1

SIMILARITY FORMULAS AND THEIR CHARACTERISTICS

Symbol and Proponent	*Procedure*	*Type of Comparison*	*Remarks*
D (Osgood-Suci, **26**; Cronbach-Gleser, **11**)	Distance measure	k (also $k-1$, $k-2$)	A general formula
CRL (Pearson, **27**)	Distance measure for standardized variates	k	
r_p (Cattell, **6**)	Transformed distance measure for standardized variates	k (also $k-1$)	Converts D to a scale from 1 to -1
Q (Stephenson, **32**)	Product-moment correlation across variates	$k-2$	Symbol Q used here instead of r for clarity
Rho (Spearman)	Correlation across scores ranked within a profile	$k-2$	
Tau (Kendall, **25**)	Based on rank arrangements	$k-2$	Highly correlated with rho
r_{ps} (du Mas, **13**)	Based on tally of similarity of slope along profiles	$k-2$	Estimate of tau based on partial data

sider person X with standard scores 1.0, 1.0, on two variates, and person Y with standard scores 1.1, 1.1. For this pair, D is $\sqrt{0.02}$. S for each person is zero, the denominator is small, and r_{in} is -1. In other words, this pair of persons is reported by intraclass r to have maximum dissimilarity, whereas the D measure reports them to be close together. The definition upon which the D measure is based appears to present a more satisfactory conception of similarity than the definition embodied in the intraclass measure.

Treatments in $k-2$ space. Several formulas have the effect of measuring similarity in $k-2$ space. We have already noted that a Q correlation based on raw scores gives the same result as obtaining D from scores standardized within profiles. Correlation is thus a special case of the D measure.

Measures of similarity are at times based on scores ranked from highest to lowest within the profile. The correlation of two such sets of ranks yields rho, which is thus like Q in many of its properties. Sometimes rank correlations are used in the belief that assumptions regarding the test score metric are thereby avoided. This is not the case. When scores are ranked, the separation between two adjacent ranks is fixed over the whole range, forcing all profiles into the same rectangular distribution. This forcing may be justified in certain studies, but it does involve a definite assumption.

Kendall's tau is a rank correlation based on the *direction* of differences between all possible pairs of variates. Tau is very closely related to rho but is somewhat more laborious to compute. In some statistical work, it is an advantage that the sampling distribution for tau is known.

Du Mas has suggested the coefficient r_{ps}. Kelly and Fiske drew our attention to the fact that r_{ps} is a sort of approximation to tau, in which pairs of adjacent variates only are considered. Results from du Mas' method therefore depend upon the order in which variates are listed in a profile. Different results would be obtained if some other order were used. r_{ps} is biased when the arrangement of traits is not strictly random. Furthermore, it uses relatively little of the information in the profile, and is therefore inexact. r_{ps} does not appear to have advantages over rho or D''.

Should differences in elevation be disregarded? A basic question is whether similarity between score sets is more meaningfully investigated by allowing differences in elevation to affect the result.

Cattell (**6**) and du Mas (**14**) have argued that differences in level between profiles are generally important and should be included in the index. For many studies, it is surely desirable not to regard two people as similar if their profiles have the same shape but differ in elevation. In the Wechsler test, for example, the elevation, being the sum of the scores, is a measure of over-all ability. The interpretation of the profile shape is dependent upon elevation. The fact that Vocabulary is higher than Digit Span means something qualitatively different for a college graduate with an IQ of 120 from what it means for a 10-year-old with an IQ of 100. To reduce the data by leaving elevation out of account may cause people to appear similar who are quite different in the domain the investigator desires to study.

On the other hand, there may be studies in which the elevation component is of no interest. If, for example, data are obtained from a personality questionnaire in which a person re-

sponds *yes* or *no* to each item, and the total score in each category is the number of questions marked *yes*, the differences in elevation between persons will be due partly to a response set (**9**). The investigator may decide that this "*yes*-saying tendency" is irrelevant to his problem, and if so, he will want to eliminate that component from his data. If he makes such a decision, reduction of the data to $k-1$ space is appropriate.

The elevation component in a profile represents the sum (or average) of all scores, and depends on the direction of scoring of the variates. A trait could be scored as "submission," for instance, instead of "dominance"; any such reversal alters the composition of the elevation score. If there is no particular reason for scoring each variate in one direction rather than the other—and this is generally the case unless variates are systematically correlated—then the elevation component is determined arbitrarily by these scoring decisions. It is highly undesirable to eliminate the elevation component when it is thus arbitrarily defined.

If a general factor is present in the variates it is often possible to choose a direction for scoring each variate which yields consistently positive intercorrelations among variates. The elevation factor, when this set of scores is used, will be heavily loaded with the first principal component of the scores, i.e., the general factor (**23**). This first factor may be an important one to consider in judging the similarity of profiles.

In general, it appears undesirable to eliminate elevation unless the investigator can interpret it definitely as representing individual differences in a quality which he does not wish to take into account in his similarity measure. If he is uncertain as to which is the more appropriate direction for scoring each of the variates, then the investigator should use the measure D in k space. Ebel (**15**), working on the problem of similarity of score sets as it is encountered in studying the reliability of rating, makes a similar recommendation. In his problem, the mean level of ratings assigned by each rater is comparable to our elevation. He lists practical considerations which make it wise at some times, and unwise at others, to consider differences in level in assessing the agreement of raters.

Should differences in scatter be disregarded? Any treatment which equalizes scatter of profiles before computing the difference measure is equivalent to projecting points onto the surface of a hypersphere within the $k-1$ space. This has the effect of increasing the jaggedness of profiles which are relatively flat, or, we might say, of reducing the jaggedness of profiles having a large amount of scatter. This introduces a serious difficulty. Figure 1 illustrates the fact that in projection onto the sphere differences between persons near the center are much magnified. The small D'_{12} becomes a large D''_{12}. D''_{34}, however, is little greater than D'_{34}. Points 1 and 2, near the center of the sphere, represent persons with flat profiles. Persons who would be judged quite similar in k or $k-1$ spaces are sometimes reported as markedly dissimilar in the $k-2$ measure.

Another aspect of the same problem is illustrated in Fig. 2. Any profile contains some error of measurement so that the location of the individual in $k-1$ space is only approximate. We indicate the possible positions in $k-1$ space of each individual over many trials by a cloud of points within the circle. The possible positions a person might take in $k-2$ space are then indicated by

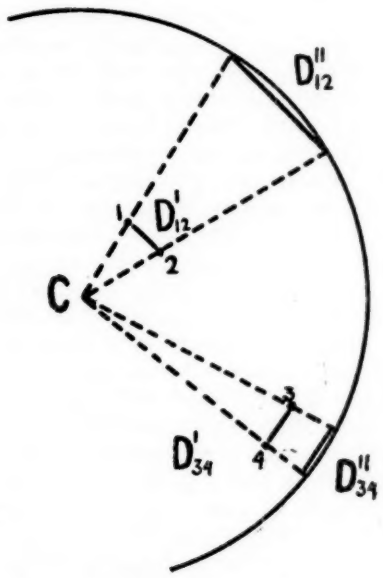


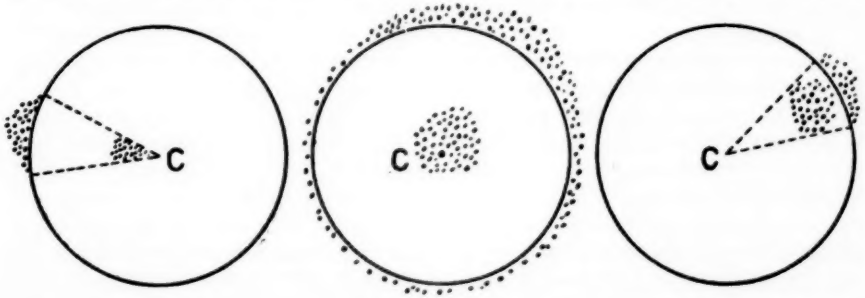
FIG. 1. MAGNIFICATION OF DISTANCES IN PROJECTION ONTO SPHERE

the distribution of points on the edge of the circle. It is clear that the greater the error, the greater the dispersion in $k-1$ and $k-2$ spaces. For a person who has a moderate amount of error and whose scatter is low, the projection in $k-2$ space has almost no meaning. On different trials he might fall anywhere in the $k-2$ space, and it is a matter of chance which persons he is similar to in a particular set of data. Either a high or low value of Q can arise by chance.

Results from analysis of profiles in $k-2$ space are dependable only when scatter is large relative to the error dispersion for the individual. So long as some profiles may be expected to be flat or nearly so, treatments of these profiles in $k-2$ space will be very much influenced by random error. This difficulty is greatest when most of the variance in the k scores comprising the profile is accounted for by a small number of factors. As more factors are represented in the variate set it is less likely that flat profiles will be obtained.

We must question whether the study of profiles in $k-2$ space, or more specifically whether correlation between profiles, is a justifiable line of investigation. This procedure has the disadvantage of removing the elevation factor and in addition tends to magnify error variance. In general, therefore, we would regard treatments in $k-2$ space as inferior to treatment of the data by D or D'.

Such success as Stephenson and his followers have obtained despite these difficulties may be explained by precautions Stephenson has introduced into his design. For one thing, Stephenson has always employed a large number of variates, each one being an item describing some per-



Low scatter, low error Low scatter, moderate error High scatter, moderate error

FIG. 2. EFFECT OF ERROR AND SCATTER ON THE PROJECTION ONTO A SPHERE

sonality trait. If the item intercorrelations are not generally positive, the first component removed as an elevation factor is a relatively small proportion of the total variance or information in the profiles. The part removed may be an important portion, but the $k-1$ profile still contains a great deal of useful information. The large number of variates also makes flat profiles in $k-1$ space less frequent.

In Stephenson's "balanced design questionnaire" each item is accompanied by another statement which has approximately the opposite meaning. By this device, Stephenson essentially assures that the sum over all items (i.e., elevation) departs from zero only by chance, and thus no information is eliminated from the data during the statistical elimination of the elevation component.

The magnification of error in projection to $k-2$ space will be slight if few persons have flat profiles. This can be assured by introducing items which have unequal means for the group. Then the centroid of the group will be far from the center of the sphere on which persons are projected. The difficulty with this solution is that, as the centroid of the group moves farther from the center of the sphere, persons are less differentiated in $k-2$ space, and error accounts for a larger proportion of the dispersion.

It is not surprising that most profile studies today utilize comparisons in $k-2$ space, since the problems have been conceived in terms of correlation as used to study relationships between tests. It is questionable, however, whether that model is a particularly good one. In determining the similarity between two *tests*, it is reasonable to eliminate the mean and variance from consideration. As Thomson (**34**) and Burt (**4**) have pointed out, the test mean represents its general level of difficulty for the population, while the variance is a function of the units used. Differences between tests in these values are usually quite arbitrary, depending on the choice and number of items. When we are mainly interested in the underlying relationship between tests these differences are of no importance and are neglected in the correlation formula. In dealing with similarity of *individuals*, however, it is necessary to consider rather carefully what logic is involved when individuals are equated for level and scatter.

Measures in $k-2$ space can give useful information only if both the dispersion of persons in $k-1$ space and the scatter for nearly all persons are large relative to the error dispersion. Data in $k-1$ space are required to determine whether these conditions are met. Then one can determine whether profiles in $k-1$ space are reliable, and whether there are many flat profiles. The investigator can, if he wishes, eliminate the people with flat profiles from the study. The forced-sort does not collect data on scatter, and one has no basis for judging which profiles are reliably located.

It seems quite important for those studying similarity to investigate reliability directly by obtaining two estimates for each profile. Reliability of $k-2$ space measures has ordinarily not been examined in past investigations of similarity.

In those studies where $k-2$ space measures have been used in the past, properly interpreted positive results need not be discounted. The faults to which we have drawn attention operate to obscure true relations and to make the measurement technique insensitive. This would make nonsignificant results likely in some instances where a better technique

would find more relationship. It would tend to make particular Q correlations or differences between such correlations undependable and inconsistent.

In summary, our consideration of all possibilities leads us to the opinion that the most generally advisable procedure for comparing profiles is to employ D in k space, except where it is known that the elevation factor is saturated with a variable which it is desired not to consider.

Contribution of Each Variate to the Similarity Measure

The Mahalanobis distance. A formula which we have not discussed to this point is the generalized distance measure of Mahalanobis (see Rao, **30**). The Mahalanobis distance is found from the formula:

$$\mathbf{D}^2 = \sum_j \sum_{j'} \alpha^{jj'} \Delta x_j \Delta x_{j'} \qquad [7]$$

where $\alpha^{jj'}$ is the jj' element of the inverse of the covariance matrix between variates within groups. We use **D** to distinguish this measure from our D. The Mahalanobis measure was designed for the purpose of measuring the distance between groups, rather than between individuals, but the formula can also be interpreted as related to the difference between individuals. If this is attempted, the intercorrelations of the variates for an appropriate reference group must be known.

The **D** measure is a measure of similarity in which the orthogonal components of the original set of variates are assigned *equal weight.* In other words, the complex formula presented above yields the same results as would be obtained if one factored the correlation matrix into k orthogonal factors, computed the person's scores on these components, and then applied the D formula to measure similarity. For variates which are standardized and uncorrelated **D** is identical to D.

D has several interesting properties. It has a known distribution function and thus forms a basis for testing the significance of a difference between groups. Moreover, **D** is closely related to Fisher's discriminant function, and particularly to the proportion of individuals classified into the wrong group by the most efficient possible discriminant function (**30**, p. 180). It is not, however, especially suited to the descriptive problem which we are discussing.

In any set of correlated variates, some variance is due to general qualities or factors represented in several variates, some due to meaningful factors found only in a single variate, and some due to error of measurement. In a principal-components analysis, k factors will be determined but the last factors may be almost entirely due to error of measurement. The Mahalanobis measure weights unreliable and unimportant factors equally with the first few components in the variates. That is, it assumes that any k variates represent k equally important factors. This is undesirable in a descriptive index, since differences between individuals on factors which are not well represented in the test battery will be unstable from one trial to another, and hence **D** for individuals will be unstable. When the formula is applied to differences between groups, no such problem arises, for groups will show negligible differences on factors which consist largely of error.

Weights in the D measure. The interpretation of the D measure is facilitated if we consider what weight it assigns to the orthogonal components underlying the variates. Some investigators have proposed that uncorrelated scores be employed in any

study of similarity. We find, however, that a meaningful interpretation can be made when D is applied to correlated variates.

First, we may note that when the variates used in formula [1] are uncorrelated, they contribute to D^2 in proportion to their variances. Hence the investigator who standardizes his variates is assigning equal weights to them, and any difference in variances assigns greater weight to some of the tests than to others. When variates are correlated, D^2 is dependent not only on the relative variances of the variates used, but also on the configuration of the variates in the factor space.

In order to obtain some insight as to the weighting of factors resulting from the use of formula [1] on correlated variates, let us consider first the case in which all variates are standardized. Then D^2 computed from such standardized scores is identical with that obtained if one were to determine the principal axes of the test configuration, compute each individual's score on each of these components, and then weight these component scores by the square root of the latent root for that component before computing D^2.[4] The principal-axis solution is a method of factor analysis which removes as much of the variance as possible in each successive factor. The latent root corresponding to each factor reflects the proportion of variance that is accounted for by that factor. *Thus, D^2 weights factors according to their representation in the test configuration.*

When an investigator employs a group of correlated variates, the factors represented most frequently among his measures are often especially important to the problem under investigation. If the D measure were applied to a Wechsler profile, for example, the general factor running through the variates would have higher weight than any more specific element found in only one or two subtests, and this might be wholly desirable.

The relatively large weight assigned to the first principal component must be considered in interpreting results even of data gathered by means of the Q sort where elevation per se has been eliminated. Rogers' work will serve as a convenient example of this possible difficulty. He had a patient describe herself and her ideal by Q sort before and after

[4] The following demonstration of this relationship is based on C. Harris' suggestion (21) that properties of D can be studied by describing the measure in matrix notation. Let us define the matrix S as the array of standardized scores of persons, where columns pertain to individuals and rows to tests. $S = FX$ where F is the matrix of factor loadings of the tests obtained by the principal-axis method and X represents the matrix of subjects' standard scores on the factors. Then if F is nonsingular one can obtain the X matrix from $X = F^{-1}S$.

Suppose, however, we weight the factor scores by the square root of the appropriate latent roots. Let L signify the diagonal matrix of latent roots. Then

$$L^{1/2}X = L^{1/2}F^{-1}S$$

and

$$(X'L^{1/2})(L^{1/2}X) = (S'F'^{-1}L^{1/2})(L^{1/2}F^{-1}S)$$
$$= S'F'^{-1}LF^{-1}S.$$

Since $F'F = \mathrm{L}$, for a principal components solution,

$$(X'L^{1/2})(L^{1/2}X) = S'F'^{-1}F'FF^{-1}S = S'S.$$

Now Harris has shown that D is obtained from $S'S$ by adding any two diagonal entries representing two persons and subtracting the corresponding off-diagonal entries. Performing this operation on the matrix $X'L^{1/2}L^{1/2}X$ gives the same result as an operation on $S'S$ itself. Therefore D from factor scores weighted by square roots of latent roots is identical to D from standard scores on tests. If Harris' operation were performed on the matrix $X'X$, the result would be the Mahalanobis measure **D**.

therapy (**31**). He found that the pre- and posttherapy selves were not highly similar, that the two ideals were closely related, and that the Q correlation between self and ideal was increased after therapy. This might be interpreted as a change in the structure or configuration of personality. If, however, many of the items express a general "adjustment" factor, then there is a strong common bipolar factor running through the items. This factor will have large weight in the Q correlation. We therefore cannot be sure whether the results in Rogers' study are due to *configurational* changes in the personality of his subject, or due merely to her increased willingness to describe herself as well-adjusted.

The recognition that the D measure allows greater weight to factors which are represented more strongly in the score set emphasizes the importance of choosing the original variate set with care (**16**). In studies where the variates are assembled as a random collection of items, there is considerable danger that the weights assigned to the various psychological components will not be fully appropriate.

In our discussion to this point we have assumed that variates are standardized. In Wechsler profiles, for instance, this is accomplished by the use of a standard score scale for each subtest. In the majority of investigations of profile similarity, similarity has been determined from raw scores on tests or items. The contribution of each principal component to D^2, when unstandardized variates are used, is proportional to the corresponding latent root of the covariance matrix between variates. This means that the contribution of any component to the D measure depends upon the number of variates in which it appears, its loading in those variates, and the variance of the tests in which it appears.

In many studies the first principal component will have a weight substantially greater than that for the remaining components. While the investigator may be willing to let the weights on the lesser components fall out by chance, he may have a specific reason for desiring to reduce the weight given to the outstanding first component. In a study of the similarity of persons in the domain of adjustment, for instance, he may wish to group people more nearly according to the character of their complaints than according to their degree of adjustment. This degree of adjustment is likely to loom large as a factor in a set of adjustment measures, however. We therefore suggest the possibility of computing an elevation score for each person, and determining a new measure D_w:

$$D_w^2 = D^2 - k(1 - w)\Delta^2 El. \quad [8]$$

Here, the weight w can range from zero to 1, with the extreme values yielding D'^2 and D^2, respectively.

Before leaving this subject, we should note that the weights of variates in D' are proportional to the contributions of the principal components to the variate set after the elevation factor is eliminated. The elevation factor is usually very nearly the same as the first principal component if variates are positively intercorrelated. The transformation of data to eliminate scatter, which is involved in treatments in $k-2$ space, produces substantial alterations in the intercorrelation of variates. For this reason, the factors which account for most of the variance in k and $k-1$ space may not be the same as the principal components in $k-2$ space.

Our recommendation on the basis of all the foregoing considerations is

that the investigator may properly use D or D_w, whether variates are correlated or not. He should give careful thought to the question of whether or not to standardize variates. In many studies of similarity it is probably desirable to perform a factor analysis on the matrix of correlations or covariances among tests before studying similarity of persons. This permits the investigator to select his set of traits or their weighting on a more intelligent basis than he could without the factor analysis.

Cluster scoring. It may often be desirable to employ many items to measure a much smaller number of traits. This is the plan used in assembling items for many tests (e.g., Kuder, Guilford-Martin). Consideration should therefore be given to special problems arising for such a set of items. A particularly important question is whether the *items* should be treated as variates in the D measure, or whether scores on *clusters* of items (i.e., subtests) should be used.

When assembling groups of items to measure particular traits it is difficult for the investigator to make sure that these traits will have the desired weight in the D measure based on item scores. The principal components of the items will not be the same as the intended traits. Each trait will be a complex and unknown combination of the principal components. Its weight will depend highly on the choice of items and their particular factor structure.

The investigator has several possible procedures which may help him to approach the desired weights. Stephenson has suggested constructing items which systematically sample the domain of traits under consideration (**32**). If this sampling were perfect, he would insure uniform coverage of the domain so that the traits would be uniformly weighted. This approach is likely to succeed only if the item writer has more knowledge of the factorial structure of personality items than is presently available. Another solution is to perform a factor analysis on the set of items, then rotate to the desired factor solution and obtain trait scores on which to compute similarity measures. This, however, is generally impractical.

In some cases a more practical solution is to combine items into groups or clusters and obtain subtest scores for each person. Such cluster scoring is feasible only when there is a logical or statistical basis for combining items. Cluster scoring may be based on a priori grouping of items, but these groupings should be analyzed for internal consistency. From the matrix of intercorrelations of the pool of items, it would be possible to assign items to relatively homogeneous subtests (**12**).

D based on cluster scores weights the underlying components of the items differently from D based on the original items. In the cluster distance, the element common to the several items is given greater weight than it has when the distances on the separate items are combined. The sum of a group of items gives relatively great weight to factors present in more than one item (**10**, **23**). If specific factors each present in only one item are not especially important, cluster scoring reduces their combined weight in order to give greater weight to the common element running through a whole group of items. To give a specific illustration, a score on hypochondriasis or health adjustment based on a number of items will give great weight to a general tendency to claim somatic symptoms. It will give less weight

than the item scores to specific symptoms such as a tendency to have colds or to have headaches.

In the same manner that cluster scoring reduces the weight given to specifics, it also reduces the weight given to differences between persons arising from error of measurement. Hence cluster scores, and similarity measures based on them, will be more reliable than scores based on the items. Warrington (**36**), with his hypothetical data, has confirmed this greater dependability of cluster-scored profiles. For one particular criterion, for instance, using *Q*-sort data, he found these validity coefficients:

D measure based on items as variates, perfect item reliability .70
D measure based on clusters as variates, perfect item reliability .74
D measure based on items as variates, moderate item reliability .18
D measure based on clusters as variates, moderate item reliability .66

It is apparent that cluster scoring overcomes much of the loss of information due to item unreliability. Stephenson is now essentially using cluster scoring in his analysis of variance based on the *Q* sort (**33**).

Cluster scoring has an interesting effect on data gathered by means of a *Q* sort. In this case even though individuals cannot differ in scatter over the total set of items, their subtest profiles can differ widely in scatter. Thus it is possible for some persons to have flat cluster profiles and others to have a high degree of scatter. This results because cluster scores utilize considerably fewer degrees of freedom than are implied in the item profile.

Some Short-Cut Formulas

In the course of our investigation, we have discovered the possibility of developing short-cut formulas for studying groups of persons. These are not entirely satisfactory, because they are based on the average of D^2 over a set of pairs. In general, D provides a better metric than D^2 for studying similarity, since large distances are much magnified in squaring. The following formulas may nonetheless be useful as a first rapid way of answering questions about groups. The formulas also provide insight into the nature of distance measures, since factors which increase mean D^2 will also in general increase mean D and median D. The formulas are particularly useful as a tool for checking computations.

In any group, the mean distance between persons over all pairs of persons in the sample is

$$\overline{D_{ii'}^2} = 2\,\frac{N}{N-1}\sum_j V_j. \qquad [9]$$

V_j is the variance, equivalent to σ_j^2. This is an expression for the homogeneity of a group or its dispersion. If we take one-half the mean D^2 within the group, we obtain the mean dispersion (distance squared) from the centroid of the sample.

The average D^2 of an individual i from other members of this Group Y, is obtained from

$$\overline{D_{ii'}^2} = \frac{N}{N-1}\,(O_Y P_i^2 + \sum V_j). \qquad [10]$$

Here i' varies over all other persons in Group Y, O_Y is the centroid of the sample, and $O_Y P_i$ is the distance from i to this centroid. O has the coordinates $\bar{x}_j$, the average for j in Group Y. If i is not a member of Y, the coefficient $N/N-1$ is dropped to get the average D^2 from i to all members of Y.

The average D^2 between members of two groups, that is, the average when each member of one group is paired with every member of the other is

$\overline{D_{ii'}^2} = \overline{O_Y P_i^2} + \overline{O_Z P_{i'}^2} + O_Y O_Z^2 (i = 1, 2, \cdots N_Y; i' = 1, 2, \cdots N_Z)$. [11]

Here we see the average cross-similarity as made up of three components: squared distance between group means, dispersion within the first group, and dispersion within the second group.

Conclusions

Studies of similarity between sets of scores have used a large number of techniques for assessing similarity. The most satisfactory model appears to be to conceive of the tests as coordinates, and each person's score set as a point in the test space. Then distances between points, computed by the D measure, are an index of similarity between score sets. This measure is a general one, to which other common techniques such as Q correlation can be related. These other techniques frequently disregard or distort some of the information in the data, in ways which may be undesirable in a particular study.

The investigator of similarity must give particular attention to his choice of variates. The similarity measure depends on the content of the variate sets, on the scales used for measuring the variates, on the choice among possible similarity indices, and upon the decision whether to score separate variates or clusters of variates (i.e., subtests). The similarity index gives especially large weight to the first principal component among the scores or items, and therefore may be relatively insensitive to the shape or configuration of profiles. On the other hand, techniques which leave the elevation of the profile out of account are usually undesirable. A formula for a weighted similarity index is offered to reduce any overemphasis on the first component.

Many commonly used operations, including the Q sort and product-moment correlation between persons, ignore differences in scatter between profiles. It is not generally desirable to do this, especially because if any profiles are relatively flat, the similarity indices involving them will be highly unreliable. The loss of information about differences in scatter may also be undesirable on theoretical grounds.

It is most important that any investigator understand the assumptions and limitations of whatever technique he employs to study similarity. Different treatments will yield different conclusions. In many studies, the most appropriate technique will be to apply the formula for D or D_w to profiles based on clusters of items.

Profile research is necessarily faced with many difficulties. In spite of these, it is our hope that the adoption of techniques which include as much information as the data provide, and which do not introduce additional errors of their own, will permit studies of similarity to advance psychological knowledge.

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HARRISON AND HARRISON'S MODIFICATION OF THE MÜLLER-URBAN WEIGHTS

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In a recent paper Harrison and Harrison (**2**) suggest that a modification of the Müller-Urban weights used in the psychophysical method of constant stimuli should be generally applicable in psychophysical measurement.

An experiment recently conducted in our laboratories raises the question whether such a modification is worth the additional computational labor. Purposes of the experiment were to examine the respective reliabilities of, and intercorrelation between, the above modification and the classical method. Absolute thresholds for visual acuity (both eyes) were, in the latter case, obtained by using conventional Müller-Urban weights, as described by Guilford (**1**), and thresholds in the former were computed from the same data using the Harrisons' weighting procedure. In the conventional method, a standard correction for chance successes attributable to guessing was calculated for each stimulus value used.

The visual acuity target utilized in this investigation was the Landolt ring, a nonletter target consisting of a broken ring which may be rotated so as to place the gap at top, bottom, left, or right of the configuration. This target combines minimum separable and minimum visible characteristics as a critical test of acuity. The target sizes are determined by a decimal acuity series, with decimal notation defined as:

$$\text{Decimal acuity notation} = \frac{1}{a},$$

where a equals the angle in minutes subtended by the unit design of the target. In the case of a Landolt ring, unit design is the width of the gap, which is also equal to the width of the stroke. The outside diameter of the ring is five times this distance.

Subject-target distance was 34.5 feet, and surface light on the target was approximately 40 foot-candles.

Thirteen male college students were tested. Each was presented with a series of five consecutive targets, in a descending order of size. The largest target employed was so selected that the subject responded to it correctly less than 100 per cent of the time. Each subject was presented with two series of trials, in each of which the five targets were separately presented 50 times, correct responses (target gap position) being randomly varied. The two series, hereafter referred to as Data A and Data B, were presented in a single session with a separation of a five-minute rest interval.

Subjects were orally instructed to guess when uncertain. Trials were untimed, with the subjects' own rates of response setting the pace throughout the sessions. A change in response was allowed at any time prior to the presentation of the subsequent trial.

The raw data were treated in the two aforementioned manners. In employing the conventional Müller-Urban weights, a correction was made for chance successes in this four-choice test. The formula which was applied was:

$$P_c = P_u - \frac{100 - P_u}{3},$$

where P_c equals corrected percentage correct, and P_u the uncorrected percentage correct. Harrison and Harrison's modification was utilized to fit a second set of curves to the data. It was necessary to compute a table of modified weights for the 25 per cent level of chance success; this was done according to the Harrisons' procedure.

Thresholds obtained are shown in Table 1. The Pearson r for test-retest reliability was .959 for the Urban-Constant method and .943 for the Harrisons' modification. Intercorrelation between the two methods was .995 when thresholds were computed on Data A and B combined.

These data led to the following conclusions:

1. There is a very high correlation ($r = .995$) between the thresholds obtained by the two above methods. Thresholds are, by definition, dependent upon computational methods used to compute them. Therefore there can be as many "true" thresholds as there are computational methods, and one is thus neither more nor less "true" than another.

2. The respective reliabilities of thresholds yielded by these two methods are lacking in any practical differences.

3. Additional computations are necessary to prepare modified Müller-Urban weights according to the Harrisons' procedure. (*a*) Separate tables of weights must be prepared for each level of chance success encountered in any psychophysical experimentation. (*b*) Since the Harrison modification results in an asymmetric curve when the Müller-Urban weights are plotted against percentage correct, separate weights must also be computed for *each* percentage, instead

TABLE 1

VISUAL ACUITY THRESHOLDS FOR 13 SUBJECTS OBTAINED BY TWO COMPUTATIONAL METHODS

Subject Number	*Method*					
	Urban Constant			*Harrisons' Modification*		
	Data A	*Data B*	*Data A+B*	*Data A*	*Data B*	*Data A+B*
1	1.170	1.206	1.174	1.193	1.180	1.185
2	0.929	0.842	0.910	0.884	0.845	0.864
3	1.007	0.904	0.953	1.010	0.914	0.955
4	1.210	1.289	1.246	1.208	1.279	1.246
5	0.919	0.929	0.921	0.924	0.934	0.926
6	0.926	0.918	0.922	0.938	0.923	0.930
7	1.254	1.230	1.246	1.253	1.231	1.244
8	1.074	1.029	1.052	1.078	1.030	1.054
9	1.396	1.347	1.372	1.401	1.346	1.373
10	1.079	1.043	1.061	1.081	1.045	1.063
11	0.955	1.036	0.992	0.971	1.143	1.038
12	1.517	1.508	1.514	1.539	1.521	1.533
13	0.976	1.005	0.990	0.985	1.008	0.991
Mean	1.109	1.099	1.104	1.113	1.108	1.108

of for only half of the 0 to 100 per cent range, as is the case with the classical weights.

4. When the standard Müller-Urban weights are used in a four-choice situation, the corrected percentage values yield a plot in which the weights are symmetrically distributed, as may be seen in Table G of Guilford's (1) appendix. Percentage correct values range, in this case, from 1 to 99.

5. Since both measures yield a measure of precision (h), a standard deviation, and a meaningful estimate of the standard error, it appears that Harrison and Harrison's proposal lacks practicality for wide application.

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BORING, E. G., LANGFELD, H. S., WERNER, H., & YERKES, R. M. (Eds.) *A history of psychology in autobiography.* Vol. IV. Worcester, Massachusetts: Clark Univer. Press, 1952. Pp. viii+356. $7.50.

The first three volumes in this series were published in 1930, 1932, and 1936. As a result of action taken by the American Psychological Association we now have Vol. IV, which includes the autobiographies of Walter Van Dyke Bingham, Edwin Garrigues Boring, Cyril Burt, Richard M. Elliott, Agostini Gemelli, Arnold Gesell, Clark L. Hull, Walter S. Hunter, David Katz, Albert Michotte, Jean Piaget, Henri Piéron, Godfrey Thomson, L. L. Thurstone, and Edward C. Tolman. The ratio of Americans to Europeans is 8 to 7, as compared with 7 to 8 in Vol. I. At the time the authors were selected (13 of them early in 1950, the other two a year later), all but Piaget were above the age of 60 and nearing the end of their professional careers. Three have since died, Hull and Bingham before the volume was published and Katz shortly thereafter. The relative maturity of the authors is one of several reasons why Vol. IV is the most interesting and valuable of the series. Some of the surviving authors of Vols. II and III who were caught in mid-career will regret that they were not saved for a later volume. Too early selection may leave half or a third of the life story untold; and what is worse, it leaves the author tagged forever with views he may have later discarded. This reviewer shudders to recall some of the dogmatic opinions he expressed in Vol. II, particularly one concerning racial differences in intelligence. Burt had the good sense to refuse an invitation he received at about the same time.

There will be disagreement on the Committee's selection of authors, for no such list could be expected to meet with universal approval. There will probably be little disagreement in this country on the European group, except possibly in the case of Gemelli, whose work is little known on this side of the Atlantic. However, psychologists in both hemispheres will wonder about the absence of Köhler and Lashley in the American list. Their omission, the reviewer is reliably informed, was not the fault of the Committee; both were invited to contribute and both firmly refused. But other omissions will also be questioned, among them the following past presidents of the APA, all of whom were above the age of 60 and still active professionally: Dashiell, Guthrie, Langfeld, and Miles (to name them alphabetically). These, as well as several others who might have been considered, were passed over in favor of Elliott, who has done almost no research, and Bingham, who is less known for his research than for other activities. Yet both of these choices can be defended. Elliott in his long career as a brilliant teacher and departmental administrator has helped mold the careers of many able psychologists; Bingham, besides making important contributions to military psychology in two world wars, probably influenced the early history of industrial psychology in this country more than anyone else.

It is impossible to summarize here the individual autobiographies, and

to attempt an appraisal of their relative merits would be presumptuous. The reviewer has chosen instead to comment on the unusual aspects of some of the careers and to point out differences among authors in the ways they approached their task and in the kinds of information they have given or omitted.

The volume is notable for the number of authors who were migrants to psychology from other fields. Of course such migration is not a recent phenomenon; most of the founders of modern psychology were migrants, usually from philosophy but sometimes from other disciplines. About half of our 15 authors got into psychology in roundabout ways. Boring, Thurstone, and Tolman were engineering graduates, Thomson took his doctorate in physics, Gemelli in both medicine and biology, and Piaget in the natural sciences. Hull, too, entered psychology as a migrant but at an earlier stage in his education. The total impact of these men on contemporary psychology has surely been very great, but whether less or more than it would have been if they had entered psychology by a more direct route is impossible to say. On the one hand, some of them were able to apply their previous training in science to certain types of psychological problems more effectively than if they had not detoured; on the other hand, the detours in some cases were costly in time. Here are a few facts about the detours and delays experienced by this group.

Boring had only one course in psychology (that fortunately with Titchener) prior to age 24 when, after returning to Cornell for graduate work in physics, he was "caught" for a course in animal psychology that sealed his future. His Ph.D. at 28 does not suggest much retardation, but the fact is that he had spent four years as a student of engineering and another year working in a steel mill.

Gemelli, who took his M.D. degree at the age of 24 and his Ph.D. in biology at 26, was 31 when he first became interested in experimental psychology and close to 40 when this interest became paramount.

Tolman lost two years from illness and was 25 years old when he graduated in engineering at Massachusetts Institute of Technology. He had gone to M.I.T. not because he wanted to be an engineer but because of family pressure; his father had graduated from that institution and was one of its trustees. During his senior year he read some of William James and fancied he wanted to become a philosopher. He entered Harvard that summer for a course in philosophy and one in animal behavior. The latter course turned him to psychology, in which he got his doctorate four years later at the age of 29.

Thomson received his Ph.D. in physics and mathematics at age 25 and first became interested in psychology as a result of having to give a course in educational psychology to prospective teachers. When he was 30 years old he visited Myers' laboratory, where he was allowed to spend a long summer vacation working by himself through the experiments in Myers' textbook. Returning to his teaching he undertook some research on the side and before the age of 33 had his first two papers ready for publication, one on psychophysics, the other on factor analysis. The latter created a sensation when it was published two years later under the title "A Hierarchy without a General Factor." Thomson's case is unique in that he never had a real course in psychology, though he was awarded a D.Sc. in the subject by

Cambridge University on presentation of his researches and on passing an oral examination. His age at the time was 32.

Piaget took his doctorate in science at the age of 22 with a dissertation on mollusks. By that time he had decided upon psychology and for the next three years studied it first at Zurich then at the Sorbonne. Though a migrant to psychology he was not delayed, for he got his degree earlier than is usual and has managed to utilize in his psychology some of the biological concepts he had developed in his late teens.

Hull entered college at the age of 22, after two years of teaching in a rural school, and devoted most of his freshman and sophomore years to courses in mathematics, physics, and chemistry in preparation for a major in mining engineering. Then he was stricken with polio, which cost him an entire year and left him so badly crippled that he had to look about for a more sedentary occupation. While trying to reach a decision he taught in a village school for two years, read James' *Principles*, and at age 28 began his junior undergraduate year in college as a psychology major. He was 31 when be began his graduate work and 35 when he received his doctorate.

The reviewer finds the autobiographies of these migrants to psychology fascinating and provocative of questions not only on the merits of vocational guidance but also on the kinds of training useful in the making of psychologists. Readers will be amused by (and some may even profit from) Thurstone's caustic comments on the differences between his professors of engineering and some of his professors of psychology. One is led to wonder whether undergraduate courses in the physical or biological sciences may not provide a better foundation for the psychologist-to-be than do the usual undergraduate courses in psychology. At that level, perhaps, the acquisition of psychological information may be less important than training in the logic and techniques of the more exact sciences.

Gesell was not a migrant to psychology though he did make some detours before settling down to his research career. First, he taught in high schools for three years prior to taking his doctorate in psychology at age 26; then, after four additional years of teaching (three of them in normal schools), he decided that the lifework he had planned called for a medical degree, and he spent five years getting it. Thus he devoted twelve years to teaching and study over and above the time required for the Ph.D. degree. Perhaps not even Gesell could say how many (if any) of these years were really time lost; if loss there was, it seems to have been made good later by an extraordinary tempo of research productivity.

All the autobiographies have certain things in common: each tells something about the immediate circumstances that influenced the author to become a psychologist, traces the leading developments in his work, and indicates what he thinks his more important contributions have been. Apart from this common core, which varies from author to author in amount of detail and in distribution of emphasis, the autobiographies present striking contrasts. Some are almost completely intellectualistic, others are intimate and personal. Michotte illustrates the former tendency; Boring, Burt, and Tolman the latter. A majority give some information, and several of them a great deal, about ancestry, family back-

ground, and childhood environment; Gemelli, on the other hand, omits all of this and begins his life story with his last years as a university student. Thomson tells nothing about his family background and nothing about his early childhood beyond the fact that his schooling would probably have ended at the age of 13 but for a scholarship that enabled him to get a secondary education; the years after 13 are fully covered. That first scholarship, one of many he was to receive, has paid rich returns, for years later the memory of what it had meant to him motivated Thomson to devise mental tests useful in identifying gifted children who might not otherwise get the schooling they should have.

Most of the authors report no maladjustments or only minor ones, but Boring is especially frank in describing his and in showing how compensations for them have helped to shape his adult motivations. The authors also differ in the amount of information they give on rate of mental development in childhood. School acceleration of one or two years is reported by several, but only two cite much additional evidence of intellectual acceleration. Burt learned Latin and German declensions in early childhood, wrote out little sermons at 7, won a scholarship at 11, and at 15 read and made notes on Ward's scholarly article on "Psychology" in *Encyclopaedia Britannica*. Piaget at 10 wrote a one-page article that was published in a natural history journal, began early in his teens a series of authoritative articles on local mollusks, at 15 "devoured" Sabatier's book on the philosophy of religion, at 16 read Bergson's *Creative Evolution*, and by 18 had formulated his theory of part-whole relationships "in all fields of life (organic, mental, social). . . ." At 19 he wrote a philosophical novel that was later published. How many of the authors would have rated as high in IQ as Burt and Piaget we have no way of knowing.

A majority of the authors say little or nothing about their hobbies either in childhood or later. Exceptions are the lifelong interest in music mentioned by Bingham and Burt, and in art mentioned by Burt and Katz. Elliott's hobby is astronomy and Thurstone's is photography; both of these interests have persisted since childhood. Piaget early developed a succession of intense interests in natural history, some of which were more or less permanent. Gesell's interests in his undergraduate years included (besides psychology) history, public speaking, and writing; the latter two have persisted and furthered his career. Perhaps the most diverse interests were those of Burt, which included (besides music and art) the theatre, literature, archaeology, Egyptian hieroglyphics, and anthropology. In contrast, Boring's 80-hour workweek could hardly leave much time or energy for avocational pursuits. Gesell is the only author who reports interest in any active outdoor sport at any age; a few mention the fact that lack of ability in sports led them to try to excel in their studies. Five or six have enjoyed working with mechanical contrivances and two (Katz and Thurstone) have patented inventions.

Although religious interests were common among the forebears of several authors, only Michotte and Gemelli (both Catholics) indicate that they themselves are deeply religious. Gemelli's conversion to Catholicism and neoscholastic philosophy at the age of 31 seems to have influenced permanently his views on vitalistic

problems common to psychology and biology. Three of the eight American authors were either wholly or in part of Quaker parentage, a ratio so far out of proportion to the number of Quakers in the population that it is hardly to be accounted for by pure chance.

On any reasonable index of vanity these psychologists would rate as modest. So far as the reviewer is competent to judge, none of them seems to have claimed more for his achievements than the facts warranted, and not more than two or three seem to have approached that limit. Readers may find it amusing to rate the authors on sense of humor, for they run the gamut from extreme and unrelieved serious-mindedness to sustained undercurrents of humor and occasional flashes of wit.

If space permitted it would be interesting to estimate the effects of two world wars on total life accomplishments and on changes in the direction of achievement. As would be expected, the wars were far more costly to the Europeans than to the Americans in time lost. Piaget, a Swiss, was an exception to the rule. Another effect of the wars was to eliminate or greatly reduce communication across national boundaries, especially in Europe. When communications were re-established after each war a number of psychologists were surprised to find that some of the "original" theories they had been incubating just prior to or during the war closely resembled theories arrived at independently by psychologists in other countries. The volume contains several interesting examples of the *Zeitgeist* at work.

The reproductive rate for these 15 psychologists is, alas, not high enough to maintain the stock. Four of the 14 who married (Gemelli did not) had no children, and the total number produced by the other 10 was only 26. Late marriage was largely responsible; the median age at marriage was approximately 29 years, with a range from 21 to 47 years. None of the marriages has ended in divorce. Seven of the 14 were married to psychologists and evidently profited thereby professionally; at any rate two of these seven have published more books than any of the other authors (Piaget 24 and Gesell at least as many).

Rumor has it that when the Vol. IV project was proposed to the APA, some of the younger psychologists whose advice was sought by the Board of Directors questioned its merits on the ground that "the past does not matter." Let us hope that the rumor was exaggerated, for it would be ironical indeed if the time ever came when psychologists were no longer interested in what makes and motivates a scientist. It is hard to believe that many psychologists who read this book will agree with those who demurred at its publication. And read it should be, not only by every psychologist but by everybody who plans to be one.

LEWIS M. TERMAN.
Stanford University.

MCFARLAND, ROSS A. *Human factors in air transportation: occupational health and safety.* New York: McGraw-Hill, 1953. Pp. xv+830. $13.00.

This is a book for which workers in the field of aeronautics have been waiting. It is a book large in size, scope, and content. Like its predecessor, *Human Factors in Air Transport Design*, it could have been written only by McFarland. The earlier work, published in 1946, dealt with the human variables that affect the

design considerations of the aeronautical engineer. The prediction that it would soon become a classic in the field has been realized. McFarland has now turned his attention to how human factors influence the efficient operation of air transportation. The new book is a necessary addition to the complete aeromedical bookshelf and may confidently be expected to achieve the success of its companion volume.

The introduction states that: "The primary objective of this book is to improve the safety, efficiency, economy, and comfort of airline operations. It is the author's conviction that air transports can be operated efficiently with minimum risk only in so far as the human variables are understood and controlled." It is also McFarland's conviction that, "Greatest improvement can be achieved . . . by strengthening the medical, safety and personnel programs of the airlines."

It should be pointed out at the outset that this work was not written primarily for psychologists. Those who expect a treatise on aviation psychology, particularly the human engineers, are likely to be disappointed. The industrial psychologists will fare better since there is much that is of general interest to the applied psychologist, and five or six chapters on selection, training, fatigue, aging, etc. are of especial interest. The largest portion of the book is devoted to aviation medicine, more exactly *preventive* medicine in airline operations, and it is addressed rather pointedly to the air transport industry. However, the book is of far broader scope, as the author makes clear in his preface and choice of subtitle. Much of the material on industrial hygiene is applicable to occupational health and safety in other industrial fields.

The book contains an enormous amount of information and is thoroughly documented with over 1200 references. This material is drawn from the most diverse sources and has been painstakingly analyzed for its practical significance to airline operations. We have come to expect from the author the ability to put selections from such a mass of material into a form suitable for practical use. Indeed, as was true of his first book, an outstanding feature of McFarland's presentation is that it contains so many specific and down-to-earth recommendations.

The main theme of the book is safety, and the author starts with *preventive* safety in the air and on the ground. His first concern is with the pilots and flight crews in whose hands the safety of the public in part rests. He treats their psychological and medical selection, their training and indoctrination, the maintenance of their health, the necessity of periodic medical examinations, the control of fatigue, and the factors in their lives that affect efficiency and aging. Next he undertakes a similar analysis of the selection, placement, health, and safety of the supporting people on the ground. Since there are 10 people on the ground for every pilot in the air, the sickness and accident rates of ground personnel are of great economic importance in airline operations. Ground conditions also bear upon safety in the air, since the two are intimately related. Sanitary control and the prevention of infectious diseases are especially important to operations in certain overseas bases, and careful attention is given to this problem as it affects operating personnel, passengers, the transportation of diseases, and quarantine procedures. Finally, an analysis is made of the factors that operate in keeping passengers well and happy. This includes a discussion of the

special problems that arise in the air transportation of patients.

A second aspect of safety relates to *rescue* safety, and the author has made an excellent study of aircraft accidents resulting in crashes or ditching. Search and rescue practices, and survival and emergency procedures are treated in full detail.

Throughout each chapter the importance of health and safety measures in the management of airlines is repeatedly emphasized. The book concludes, therefore, with an analysis of health and safety programs that should be developed not only in the interest of the flying public but in the interest of the airlines. McFarland makes a good case for his point that well-organized medical services are profitable as well as desirable.

Occasionally the material in this book touches upon the same ground as the previous book. Where this is true, new material and a new emphasis have been added. The style is good and the material is set in large type on large, easy-to-read, double-column pages. With an eye to use as a handbook, as well as for clarification of the text, there are 171 tables, 156 figures, and an appendix of terms frequently used in the aircraft industry. Each chapter concludes with a full summary and recommendations for improvement in airline operations

J. W. GEBHARD.
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LUNDIN, ROBERT W. *An objective psychology of music.* New York: Ronald, 1953. Pp. ix+303. $4.50.

Up to the present the more important psychological texts dealing with musical problems have been Seashore's *Psychology of Music* (McGraw-Hill, 1938), Diserens and Fine's *A Psychology of Music* (Authors, 1937), Mursell's *The Psychology of Music* (Norton, 1937), and Schoen's *The Psychology of Music* (Ronald, 1940). Max Meyer's *The Musician's Arithmetic* (Ditson, 1929), though written as a text, has had a deplorably limited audience. It contains many exciting neurological speculations but is extremely difficult to read.

Seashore's book covers little more than the work of his own laboratory. It is distinctly hereditarian in flavor and favors the natural science approach. Seashore later attempted to broaden his coverage somewhat with his *Why We Love Music* (Ditson, 1941) and his *In Search of Beauty in Music* (Ronald, 1947).

The Diserens and Fine text has a better coverage but devotes what many consider a disproportionate amount of space to topics such as the possible origin of music, animal auditors, mythology and folklore, and the relation of music to magic and sorcery and to religion, melancholy, and ecstasy. The Mursell offering has been perhaps the most palatable to musicians, though psychologists have at times been distressed at the extreme Gestalt interpretation given all phenomena. Schoen's well-rounded text has the hereditarian bias of his teacher, Seashore.

Lundin's addition to this shelf of books considers most of the material treated in the earlier volumes and brings all topics reasonably up to date. A surprising omission, however, concerns Pratt's *The Meaning of Music* (McGraw-Hill, 1931). Other psychological books seemingly ignored are Schoen's *The Understanding of Music* (Harper, 1945) and the above-mentioned texts by Diserens and Fine and by Meyer.

Lundin looks to cultural explanations rather than to nativistic ones. Thus, consonance is regarded not as a phenomenon of the natural properties of stimuli but as the product of a particular culture. Music tests are

not thought to measure innate capacities. Because of their present low level of efficiency Lundin takes a dim view of them although he himself has developed a few.

Noticeable particularly in the first chapter is the interbehavioral stand of J. R. Kantor, to whom the book is dedicated. This theoretical orientation will probably confuse only slightly the psychologist who is unacquainted with Kantonian theory.

Lundin has done a workman-like job throughout the volume's sixteen chapters. He ends with a most usable bibliography of over 260 titles. In the reviewer's opinion this book should supersede the earlier texts and should greatly aid the occasional teacher who ventures to give a course in the psychology of music.

PAUL R. FARNSWORTH.
Stanford University.

KUHLEN, RAYMOND G., & THOMPSON, GEORGE G. (Eds.) *Psychological studies of human development.* New York: Appleton-Century-Crofts, 1952. Pp. xiv+533. $3.50.

This is a well-organized collection of 71 studies that have appeared in 11 books and 33 journals. More than a third of the articles appeared in a group consisting of the *Journal of Educational Psychology* and the *Journal of Abnormal and Social Psychology* (6 articles), *Child Development* and the *Journal of Genetic Psychology* (5 articles), and *Genetic Psychology Monographs* (4 articles). All articles were published in America, and all within the past 25 years.

The reports of 104 investigators are divided into 14 groupings: physical factors, learning and adjustment, sociocultural conditions, intellectual changes with age, intelligence, language-conceptual growth, interest patterns, social values and attitudes, interpersonal relationships, home and family relations, vocational orientations, and factors in personal and emotional adjustment.

In the opinion of this reviewer, the emphasis on educational and experimental studies gives this symposium a cast that may bolster its standing as a reference text for courses in child psychology but also inhibit its appeal for people hoping to achieve practical goals with children and their parents.

T. W. RICHARDS.
Louisiana State University.

BARKER, R. G., WRIGHT, B. A., MEYERSON, L., & GONICK, M. R. *Adjustment to physical handicap and illness: a survey of the social psychology of physique and disability.* New York: Soc. Sci. Res. Coun., 1953. Pp. xvi+440. $2.00.

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Conferences on drug addiction among adolescents. Sponsored by Committee on Public Health Relations of the New York Academy of Medicine. New York: Blakiston, 1953. Pp. xvi+320. $4.00.

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